105
cuk-H06309-2-p16287-6C

THE BRITISH JOURNAL OF PSYCHOLOGY

EDITED BY
JAMES WARD AND W. H. R. RIVERS

WITH THE COLLABORATION OF

A. KIRSCHMANN.	A. F. SHAND.
W. McDOUGALL.	C. S. SHERRINGTON.
C. S. MYERS.	W. G. SMITH.

Volume II. 1906—1908

THE ADVANCEMENT OF LEARNING ❋ UNIVERSITY OF ❋
LIBRARY

(2)

CALCUTTA UNIVERSITY
Sec. No.
Book No.
LIBRARY

CAMBRIDGE:
at the University Press.

LONDON: Cambridge University Press Warehouse, Fetter Lane.
NEW YORK: G. P. Putnam's Sons.
BERLIN: A. Asher and Co.
LEIPZIG: F. A. Brockhaus.
BOMBAY AND CALCUTTA: Macmillan and Co., Limited.

P16287

Cambridge:
PRINTED BY JOHN CLAY, M.A.
AT THE UNIVERSITY PRESS.

UNIVERSITY OF CALCUTTA LIBRARY. THE ADVANCEMENT OF LEARNING.

UTTA UNIVERSITY LIBRARY
Acc. No.........
Book No.........

2

CONTENTS OF VOL. II.

Part 1. *July*, 1906.

PAGE

The nature of conation and mental activity. By G. F. Stout. 1

A study of some correlations of the Müller-Lyer visual illusion and allied phenomena. By W. G. Smith. (Two Figures.) . . . 16

Immediate memory in school children. By W. H. Winch. No. II. Auditory. 52

The Wheatstone-Hipp Chronoscope. Its adjustments, accuracy, and control. By Beatrice Edgell and W. Legge Symes. (Twelve Figures.) 58

'Footrule' for measuring correlation. By C. Spearman. (One Figure.) 89

Proceedings of the British Psychological Society. 109

Part 2. *May*, 1907.

On the apparent heaviness of colours. A contribution to the aesthetics of colour. By E. Bullough. (Six Figures.) 111

The relative effects of fatigue and practice produced by different kinds of mental work. By J. H. Wimms. 153

Observations on spatial contrast and confluence in visual perception. By W. G. Smith and S. C. M. Sowton. (Seven Figures.) . 196

The vertical-horizontal illusion in school-children. By W. H. Winch. 220

Proceedings of the British Psychological Society. 226

Part 3. *January*, 1908.

PAGE

The method of 'Right and Wrong cases' ('Constant Stimuli') without Gauss's formulae. By C. Spearman. (Three Figures.) . 227

The illusion of compared horizontal and vertical lines. By G. Dawes Hicks and W. H. R. Rivers. (Two Figures.) . . 241

The influence of small doses of alcohol on the capacity for muscular work. By W. H. R. Rivers and H. N. Webber. (Five Figures.) . 261

The Wheatstone-Hipp Chronoscope. A second note. By Beatrice Edgell and W. Legge Symes. 281

The transfer of improvement in memory in school-children. By W. H. Winch. 284

The effect of practice on the perception of the Müller-Lyer illusion. By E. O. Lewis. (Six Figures.) 294

A new visual illusion of direction. By James Fraser. (Plates I—IX. [Fifteen Figures].) 307

Proceedings of the British Psychological Society. 321

Part 4. *October*, 1908.

On the difference between percepts and images. By Carveth Read. (One Figure.) 323

An investigation of the colour-sense of two infants. By William McDougall. 338

Some observations on the development of the colour-sense. By Charles S. Myers. 353

The influence of binaural phase differences on the localisation of sounds. By H. A. Wilson and C. S. Myers. (Nine Figures.) . . 363

Some data for a theory of the auditory perception of direction. By A. M. Hocart and William McDougall. 386

The 'perceptive problem' in the aesthetic appreciation of single colours. By Edward Bullough. (Plate X.) 406

Proceedings of the British Psychological Society. 464

UNIVERSITY OF CALCUTTA
LIBRARY.
THE ADVANCEMENT OF LEARNING

LIST OF AUTHORS

PAGE

BULLOUGH, E. On the apparent heaviness of colours. A contribution to the aesthetics of colour . . . 111

BULLOUGH, E. The 'perceptive problem' in the aesthetic appreciation of single colours . . . 406

EDGELL, BEATRICE. The Wheatstone-Hipp Chronoscope. Its adjustment, accuracy and control . . . 58

EDGELL, BEATRICE. The Wheatstone-Hipp Chronoscope. Its adjustment, accuracy and control. (Second note) . . . 281

FRASER, JAMES. A new visual illusion of direction . . . 307

HICKS, G. DAWES. The illusion of compared horizontal and vertical lines . . . 241

HOCART, A. M. Some data for a theory of the auditory perception of direction . . . 386

LEWIS, E. O. The effect of practice on the perception of the Müller-Lyer illusion . . . 294

McDOUGALL, W. An investigation of the colour-sense of two infants . 338

McDOUGALL, W. Some data for a theory of the auditory perception of direction . . . 386

MYERS, CHARLES S. Some observations on the development of the colour-sense . . . 353

MYERS, CHARLES S. The influence of binaural phase-differences on the localisation of sounds . . . 363

READ, CARVETH. On the difference between percepts and images . 323

RIVERS, W. H. R. The illusion of compared horizontal and vertical lines . . . 243

RIVERS, W. H. R. The influence of small doses of alcohol on the capacity for muscular work . . . 261

PAGE

SMITH, W. G. A study of some correlations of the Müller-Lyer visual illusion and allied phenomena 16

SMITH, W. G. Observations on spatial contrast and confluence in visual perception 196

SPEARMAN, C. 'Footrule' for measuring correlation 89

SPEARMAN, C. The method of 'Right and Wrong Cases' ('Constant Stimuli') without Gauss's formulae 227

STOUT, G. F. The nature of conation and mental activity 1

SOWTON, S. C. M. Observations on spatial contrast and confluence in visual perception 196

SYMES, W. LEGGE. The Wheatstone-Hipp Chronoscope. Its adjustment, accuracy and control 58

SYMES, W. LEGGE. The Wheatstone-Hipp Chronoscope. Its adjustment, accuracy and control. (Second note) 281

WEBBER, H. N. The influence of small doses of alcohol on the capacity for muscular work 261

WILSON, H. A. The influence of binaural phase differences on the localisation of sounds 363

WIMMS, J. H. The relative effects of fatigue and practice produced by different kinds of mental work 153

WINCH, W. H. Immediate memory in school-children 52

WINCH, W. H. The vertical-horizontal illusion in school-children . 220

WINCH, W. H. The transfer of improvement in memory in school-children 284

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

January 27, 1906.

PAGE
Read, Carveth. On certain cutaneous sensations 108
Wimms, J. H. The relative effects of fatigue and practice produced in the same individuals by two different kinds of work 108

VOLUME II .JULY, 1906 PART 1

THE BRITISH JOURNAL OF PSYCHOLOGY

THE NATURE OF CONATION AND MENTAL ACTIVITY.

BY G. F. STOUT.

Interaction of Subject and Object. Mental Activity and Efficiency. Complexity of Conation. Criticism of Mr Bradley's view. Conation and Motor Sensation. Conative and Affective Consciousness. Experimental Psychology and Conation.

Interaction of Subject and Object. When a man is angry his heart beats faster and his muscles become more tense. These bodily changes may be regarded, rightly or wrongly, as due to the emotion and therefore as exemplifying the action of body on mind. But such action cannot be properly called mental activity. Mental activity is no more involved in it than in the process by which a stone falling into a pool produces ripples. The quickened beating of the heart and the increased tension of the muscles would not be acknowledged by the man himself or by others as being the man's deeds.

It is otherwise if we suppose him to express his anger by intentionally striking someone whom he regards as an enemy or by writing an indigna
letter. Here there is not merely action in the sense in which it
take place in the material world; there is interaction betw
Subject as such and his Object as such. There is the
thought of a certain actual situation and of a possible
and the alteration is not only thought of but also
sufficient to constitute what I call mental activity or
change which is wanted takes place in consequenc

the activity is so far successful; otherwise it is unsuccessful. When it is successful the resulting change is a change in the Object as such due to the Subject as such. Further, the change reacts on the Subject as such. It transforms his subjective relation to the Object. After the angry man has knocked down the person he is angry with, he may still remain angry. But his anger is now partly gratified, and to that extent modified. It may even pass away and give place to pity or regret.

Suppose, next, that the angry man attempts to knock his enemy down but does not succeed. In this case also there is interaction of Subject and Object. The actual situation as known or thought of becomes different from what it was before the unsuccessful attempt. A change has taken place in it, though not the change intended. And the subjective attitude is also modified: anger, for example, becomes baffled anger—impotent rage.

Even when there is nothing that can properly be called an attempt, there may still be found all that is psychologically essential to mental activity. In remorse for the past a certain actual state of affairs is thought of, and there is also the thought that it might have been different from what it actually is. Inasmuch as the past is recognised as irrevocable there can be no attempt to change it. Yet there is still mental activity or conation; for there is a felt tendency which would develop into an attempt if other conditions permitted an attempt to be made.

Mental Activity and Efficiency. Plainly no finite process can claim to be called an activity unless it counts as a factor in determining other processes; and yet I have asserted that there may be mental activity which fails in attaining its end and which does not even issue in an attempt to attain its end—at least in any ordinary sense of the word, *attempt.* The question may be raised: How can we be justified in applying the term 'active' to processes which are confessedly inefficient? I reply, in the first place, that they are not absolutely inefficient, but only unsuccessful. A conative process which is unsuccessful is in[illegible]cient in producing that particular result which we call its fulfilment. [illegible] is not absolutely inefficient; it counts in some way as a factor [illegible] the course of events. It does so not because it is the [illegible] process which we call mental activity, but because it is [illegible]. I take it to follow from the general principle of [illegible] or the Unity of the Universe that every state or process [illegible]rmining the nature of other states and processes.

what
as a c ot constituted
presence of certain conditions of that result and the
others. Dry gunpowder in a confined space is one condition
an explosion; if a lighted match is applied to it the explosion will take place. But the gunpowder is not tending to produce the explosion before the match is applied; before the match is applied its non-explosion is not a failure or defeat on its part; and the explosion when it does take place is not a success on its part. A causal condition which is a tendency is in its own intrinsic nature different from a causal condition which is not a tendency. In both cases there is a certain process or changeable state *A* which when combined with certain other circumstances *B*, *C*, *D* will give rise to a certain result *R*. Where *A* is not a tendency or is not recognised as such we can ascertain only by inductive generalisation that *A* is one of the conditions which together with others will produce *R*. For instance, it is only through comparing and sifting a series of particular instances that gunpowder is known to explode when a lighted match is applied to it. On the contrary, where *A* is a tendency towards *R*, its being a condition of *R* is implied in its own intrinsic nature so as to be directly evident apart from inductive generalisation. Suppose a man with a box of matches standing before an open barrel of gunpowder; suppose the man to know that a lighted match applied to the gunpowder will be followed by an explosion; suppose that all other requisite conditions are known to be present except intention on the part of the man to make the gunpowder explode. So long as the man's intention is uncertain the result is uncertain. But when once it is known that the man intends to

th [illegible]

Comp [illegible]

account of co [illegible] e wi [illegible]
complex unity combining many distinguishable asp [illegible]
I also agree with him in maintaining that though some of these [illegible]
features or aspects may exist apart from conation, yet none of them ca [illegible]
exist outside of it precisely as they exist within it. I am thus bound to agree with him in denying that conation is simple and unanalysable if this is taken to mean that it involves no complexity. But the view that conation is simple may bear a different interpretation which Mr Bradley apparently does not consider at all. It may mean that the conative complex contains a simple and unanalysable element uniquely characteristic of it—an element from which the whole derives its distinctively conative character. This is, I believe, the position which Professor Ward maintains, and, on this view of his meaning, he seems to me to be right and Mr Bradley to be wrong in opposing him.

My divergence from Bradley's view is as follows. He affirms that conation involves the unity of various distinguishable 'features' or 'aspects' in a complex whole. So far, I agree. But he apparently places all these distinguishable features or aspects on the same level as components of the whole. None of them, according to him, supplies the specific character which is distinctive of conation any more than the others. Here I fail to follow him. It seems to me that there is an ingredient of the conative complex which has a privileged position, inasmuch as it communicates to the complex its distinctive character as a conation. Further, I feel bound to maintain that this component

---

[1] "Some Remarks on Conation," *Mind*, N. S. Vol. x. pp. 437 *seq.*

is an unanalysable immediate experience; it is immediately experienced in the same sense as pleasure or pain while they are being felt[1].

The decision of a question of this kind must of course depend for each of us on a critical examination of his own experience. I must content myself with supplying clues which will enable others to follow my analysis and test its validity. Such a clue is to be found in the inquiry: What becomes of the several components of a conative complex when they are considered in detachment from the whole to which they belong? Every conation includes the apprehension of an actual situation and the thought of a change in it. But these components taken by themselves in abstract isolation constitute a merely cognative complex as distinguished from a conative complex. I may have an actual situation presented to me, and I may think of a change in it without wanting the change. I may find ink in my ink-bottle, and I may think of the ink being spilt without wanting to spill and without in any way wishing or desiring that it should be spilt. Further the change thought of is not even for mere cognition what it is for cognition within the conative complex. However else it may be determined for thought within the conative complex, it must at the very least be determined as something required to satisfy a want, need, or tendency. And it is not enough that the want, need, or tendency should be merely thought of or cognised. I do not, for instance, feel a desire for food when I merely represent someone else as feeling the craving of hunger and think of food as what is required to appease it. It is necessary that I should be actually feeling the craving myself. It is not even necessary that I should be cognitively aware of the felt craving at all except in so far as such awareness is indirectly implied in the mental reference to food as something desired. Apart from this indirect cognition, the essential condition is that the hunger-craving should be felt and not that it should be cognised. It thus appears that what gives its distinctive character to conation as such is an immediate experience which does not admit of further analysis.

Let us now attempt to consider felt tendency in separation from the cognitive complex constituted by the thought of change in an actual situation. Here we are confronted by the difficulty that it never actually exists in this isolation at the level of conscious experience at which introspective analysis is possible. Indeed it is extremely doubtful whether it is so isolated in any stage of psychical development. Thus all that we can do is to make a hypothetical abstraction of felt tendency from the cognitive conditions with which it is always found united in concrete instances. If, by way of illustrative hypothesis, we suppose that it could be thus isolated, there would no longer be anything which could properly be called conation. But there would still remain that mode of psychical existence which gives to the conative complex its specific character—the character which distinguishes it from a merely cognitive complex. The felt tendency would be entirely blind; but it would still be felt tendency. The being supposed to feel it could not itself directly or indirectly recognise the tendency as such. We could only describe the experience of such a being by saying that it had a feeling of restlessness or uneasiness, more or less intense. None the less another mind might rightly recognise the restlessness or uneasiness as a tendency requiring to be fulfilled in a certain way. The nurse, for instance, might rightly recognise blind hunger in the child as a craving requiring to be satisfied by appropriate food. It would be rational for her to sympathise with its felt need; but this could not be so if it did not feel a need at all. Thus even, if we abstract completely from even the vaguest cognition of an end to be attained, and consequently from the cognition of a tendency as such, its essential relation to something required for its fulfilment still remains. But this is precisely that which as a feature or aspect of the conative complex gives to the conative complex its specific nature.

---

[1] The question whether it can be identified with pleasure and pain is discussed below, cf. p. 11.

We may reach the same result in another way. Conation does not merely exist or not exist; it has degrees; there is more or less of it. Now this quantitative graduation seems to be independent of the greater or less distinctness or fulness of the cognition of an end. The end may be determined for thought with extreme vagueness, and yet the conation may be strong and conspicuous. There is more or less of the conation according as it is more or less intense; and this intensity is strictly analogous to the intensity of pain or pleasure as immediately felt. It is the intensity which belongs to immediate experience. We may, then, infer that it is this immediate experience which gives to the conative complex its distinctive character.

Criticism of Mr Bradley's view. Let us now compare this analysis of conation with that given by Mr Bradley[1]. The vital difference is this: where I speak of felt tendency as an immediate unanalysable experience, Mr Bradley omits this and substitutes in place of it the identification of an idea with the self. Premising that all conation involves the apprehension of an actual situation and the idea of a change in it, he proceeds to insist that the idea of the change must be identified with the self in a sense in which the idea of the actual situation is not identified with the self.

---

[1] *Mind*, N. S. Vol. x. pp. 437 *seq.*

"Identification with the self" is, as Mr Bradley admits, a highly ambiguous phrase. In a sense, all ideas which enter into an individual consciousness form part of the self. Some meaning more specific than this is required. Mr Bradley's most explicit statement on this point is to be found in a note on p. 446. He there says: "We find in conation both the theoretical and practical relation of self to an object....While taking these relations as really inseparable, let us by abstraction separate them and consider first the theoretical relation by itself. In this experience there is an object for me, let us say a fruit. This object is in the first place (*a*) felt as mine, as an element, that is, in my whole felt state[1], and it is also in the second place (*b*) felt as something other than myself....Let us now, however, add and restore to our abstraction the practical relation and let us note the difference. There will be here also an idea, let us say, of eating the fruit. This idea is itself an object beside and against the first object....Now the idea, being an object, is like the first object felt as (*a*) an element in my total state, and (*b*) again, like the first object, it is felt as something not myself. But the idea is also (*c*) as against the first object felt as mine and one with me....The idea is felt as the expression of myself as against the first object, which is now in two senses something alien to me." So far as I am able to understand it, this explanation seems to miss the precise point which requires to be explained. There is a sense in which an 'idea' of a change may be felt as one with the self without the change being desired. A man on the scaffold just before the fatal drop may feel himself as about to be hanged and yet he may not wish or desire or will to be hanged. The vital difference between this case and that of the fruit is that the subject not only feels himself as about to eat the fruit but also wants to eat it—experiences a tendency to eat it. To constitute conation on the part of the self what requires to be identified with the self is not the idea of a change, but the felt tendency towards it. Mr Bradley habitually speaks of the "actual situation" as a hindrance to the self and of the self as hindered by it. Are we to take him to mean that it is the *hindrance* which makes the actual situation relatively alien to the self? If this be so, it would seem that he is tacitly presupposing felt tendency as essentially constituting the self in contrast to the actual situation. For hindrance, in any relevant sense, can only mean the hindering of a felt tendency. Where there is no felt tendency there can be no hindrance.

---

[1] I do not endorse this language. The fruit cognised by me is not as such a feeling of mine in any sense; nor is it any element, part, or aspect of my total feeling. Apart from metaphysical theories this seems plain.

We have yet to inquire whether there is any special sense in which identification with the self is necessary to conation. We have already seen that it is not necessary in the sense that the self must be felt as about to undergo the change. There is, however, another special meaning of identification with the self which requires to be considered, and, so far as I can discover, there is only one. We distinguish a relatively limited self within the totality of our own mental being. The total content of the mind at any moment consists in part of components possessing a systematic unity in which other components do not share. This is the self in the limited sense; we may call it the organised self. Other mental contexts are relatively alien to the organised self. Interpreting "identification with the self" *in this way*, we may now inquire whether identification with the self of the idea of change in an actual situation is necessary and sufficient to constitute conation.

Is it necessary? If it is so, no conation can exist which does not form part of the organised self; none can exist which is felt as foreign to it. There can, on this view, be no conation within me which is not, in the special sense, mine. But there certainly are conations which are foreign to the organised self. Consider, for instance, 'obsession' by fixed impulses. A man's organised self may revolt at the thought of committing suicide or murder; and yet he may feel an intense and obstinate craving to kill himself or those who are nearest and dearest to him. The triumph of the craving will be a defeat of the self and its suppression will be a victory of the self. In normal life the most conspicuous examples of this detachment of conation from the self are to be found in the grosser forms of temptation. We may or may not choose to apply the term *conation* in such cases. However we decide on this point, the fact remains untouched that there exist in relative detachment from the organised self complex wholes containing not only the thought of a change in an actual situation, but also a felt tendency towards this change. Hence, felt tendencies which are by no means blind can exist and be exceedingly strong in relative detachment from the organised self. When we omit from a conative complex identification with the self in this special sense, what is left is not a merely theoretical as distinguished from a practical relation. There may still be felt tendency which is distinctively conative in character and not cognitive. On the other hand, the mere appropriation of the idea of a change by the organised self is not only not necessary; it is not even sufficient to constitute a corresponding conation, having the change for its object. The idea of a man's approaching death may be intimately interwoven with the general system of his ideas; it may so fill his mind that the affairs of his ordinary life appear relatively alien to him; and yet he may not want to die. In order that conation may exist, what is identified with the organised self must be not merely the idea of an actual situation being other than it is, but also the felt tendency to have it other than it is.

I fully admit that a felt tendency may be primarily conditioned by the fact that an idea forms part of the organised self. Whatever disturbs the unity of the organised self is *ceteris paribus* an object of aversion and its removal is desired. But this Herbartian explanation is very far from covering all cases of conation; and even in the cases which it does cover we must distinguish conation from the conditions which evoke it. The conation exists only as including the immediate experience which we have named 'felt tendency'—an immediate experience which is ultimate and irreducible, having an intensity peculiar to itself quite distinct from the degree of coherence of an idea with a system of ideas.

Finally, I would urge that any explanation which in order to give an ultimate account of conation presupposes the existence of developed mental systems puts the cart before the horse. The growth of mental systems, and therefore of the organised self, depends on interest—on felt tendencies and the pleasures and pains connected with them. But if the self as an organised system presupposes conation, it cannot be regarded as an ultimate precondition of conation.

Conation and Motor Sensation. Conation is intimately connected with the sensations which are due to the use of the muscular apparatus—including muscles, joints, and tendons. The connexion is so close and universal that many psychologists have been led to identify the immediate experience which gives to conative consciousness its specific character with motor sensations[1] of strain, pressure, and tension. This view appears to me to be quite untenable. If it were true, the intensity of conation would be simply identical with the amount of motor sensation connected with it. But this is not so. Conation may be as strong in giving the finishing touch to a house of cards as in lifting a heavy weight; it may be as strong in working out a chess problem or in bringing off a delicate cannon at billiards as it is in a football scrimmage. This point may be illustrated by experiments in which the aim is to observe motor sensations. In order to observe sensations of strain we may take a heavy weight in the hand and let the arm hang down by the side. Conation here is present in the form of a desire to know more about tendon sensation. But tendon sensations do not themselves enter into the constitution of the conative consciousness. The desire to know about them does not become more intense as they become more intense. The truth is that motor sensations have no more intimate affinity with felt tendency than visual sensations or sound sensations. Their special connexion with conative consciousness is due to their being the sense experiences which are most immediately and uniformly under our control. In general, we do not explicitly distinguish between those results of our activity which can be habitually counted on as a matter of course and the activity itself. Thus in throwing a stone the experiences connected with the movement of the hand and arm are regarded by us as an integral part of our own action, but the movement of the stone is apprehended as an effect. Similarly in writing, so long as the pen does not splutter or otherwise become unmanageable, not only are the motions of the hand but also those of the pen included in what I regard as my own action in distinction from its results. I say that *I* write, not that I make my pen write. Now the sensations which are most fully, directly, and uniformly at our command are motor sensations. We can always have these when we are interested in having them, and discontinue them when we are interested in discontinuing them. Other changes in our sense experience are produced by us only indirectly and conditionally. We can determine them only through our bodily movements. In the beginning of mental development motor sensations are very imperfectly under control, as is shown by the behaviour of children. But the child gradually acquires a constant and habitual command over groups and series of motor experiences so as to count on them as a matter of course whenever he is interested in having them. The motor experiences then cease to be distinguished from the subjective activity which initiates them. They blend with conative consciousness in the single unanalysed experience of 'motor activity.' But the word 'activity' is primarily and strictly applicable only to one ingredient of the total experience—conation. The motor sensations as such are passive results. We are active in 'making an effort'; but the activity lies in the 'making' and not in the 'effort.' The effort is something *made*.

---

[1] "Motor sensations" is an abbreviation for "sensations directly connected with the use of the motor apparatus." I assume, of course, that these are all due to peripheral stimulation.

Conative and Affective Consciousness. The prevailing view among experimental psychologists is distinctly adverse to the existence of any peculiar kind of immediate experience distinctively characteristic of conation. What we have called 'felt tendency' seems to elude introspection as carried out under the test conditions of the laboratory. The experimental psychologist when he looks for this finds only motor sensations and the qualities of affective consciousness—pleasure and pain. Professor Titchener, for example, asserts confidently that if there is any other kind of 'elementary mental process' besides sensation, it must consist merely in 'affection.' Affection is the only claimant "which has been found worthy of serious consideration by experimental psychology[1]." Titchener himself "inclines to the view (1) that affection is a conscious element distinct from and ranged alongside of sensation in the composition of consciousness," and (2) that affection has two qualities only, those of pleasantness and unpleasantness.

On this view, affection with its two qualities of pleasantness and unpleasantness is the only elementary process which we are justified in distinguishing from sensation. It would seem to follow that if Titchener and those who agree with him are right, we must be wrong in assigning a distinct place to 'felt tendency.' Probably, however, the divergence between us is less serious than it appears. What Titchener would deny on behalf of experimental psychology is the existence of felt tendency as an 'elementary process' distinct from and ranged alongside of affection, as affection is "an elementary process distinct from and ranged alongside of sensation." But I am in no way bound to assert this. On the contrary, I am inclined to agree that affection and felt tendency are so intimately connected that we cannot appropriately speak of affection as one process and of felt tendency as another. It is very difficult to find satisfactory language to describe the facts, and whatever terms we may select must in part have their meaning determined by their application. It seems, however, better to speak of distinguishable characters or aspects of the same process than of two distinct processes. From one point of view a process may be correctly named affective, inasmuch as it is characterised by the affective qualities of pleasure or pain. From another point of view the same process may correctly be named conative, inasmuch as it is characterised by felt tendency towards the alteration or maintenance of the pleasant or unpleasant situation. If a term is required to express the unity of conative and affective characters in the single process the term *interest* seems most suitable.

---

[1] *Experimental Psychology*, Vol. I. p. 91.

I admit, then, that affection and felt tendency are not to be distinguished as separate elementary processes. But I cannot concede that they are not distinguishable modes of being conscious. I cannot do so because on this assumption no adequate analysis of the conative complex is possible. Consider, for instance, a state of desire. This involves the apprehension of an actual situation as alterable, and the change must not only be thought of but also wanted or desiderated. The vital question is whether we can account for this 'wanting' in terms of affection merely, without introducing felt tendency? The most plausible suggestion is that the change is wanted when pleasure is felt in the anticipation of it. But this account of the matter is irreconcilable with the fact that desire is frequently a painful state. No one will contend that wanting a change consists in feeling *pain* in the anticipation of it. Nor is it enough that pain should be felt in reference to the actual situation. For the actual situation is often not unpleasant in itself but only because the change is desired. Desire, therefore, is presupposed as the essential condition of the pain, and the pretended analysis involves a vicious circle.

In general, the pleasures of success and the pains of failure are derivative from conative consciousness and cannot therefore constitute it. There is a single process which is at once conative and affective. But the affective quality varies according as conation is hindered or furthered while the conative attitude persists. And it cannot be maintained that what we call the conative attitude is something which involves no peculiar immediate experience. For the pains of failure and the pleasures of success are felt as such. Failure is felt as failure and success as success. But this is possible only because conation also includes the immediate experience of felt tendency.

Experimental Psychology and Conation. In conclusion we may inquire why the existence of felt tendency has for the most part eluded the systematic introspection of experimental psychologists. One reason is that they have not expressly applied their peculiar methods to this special problem. They have not, for instance, subjected processes of desire to introspection under test conditions experimentally adjusted and varied according to a systematic plan. There are obviously very great difficulties in the way of such an undertaking, and it may be that these difficulties are insuperable. However this may be, the fact remains that experimental psychologists have not applied their distinctive methods to the analysis of the conative complex as they have applied them to the analysis of complex sounds or odours.

If felt tendency exists at all it can only be detected within the unity of the conative complex. There is no possibility of attending to it separately in the way in which sensations can be separately attended to. It is known only as being that partial feature of the conative complex which communicates to the whole its distinctively conative character. Our previous analysis shows that it is one constituent of the complex among others and not merely the mode in which the constituents are combined. Further, the unity of the whole cannot be externally produced by any combination of items previously existing in detachment from it, as a complex odour can be produced by a combination of simple odours or a complex tone by a combination of simple tones. Given the conative complex in its unity we can by analytic comparison distinguish within it partial features which may also, in a sense, exist outside it. But their peculiar connexion within the complex and the peculiar qualifications which belong to them in virtue of this connexion cannot be superinduced on them by any constructive process, such as might be applied in a laboratory experiment. Finally, the element of felt tendency which so to speak leavens the whole, cannot be observed at all in detachment from the conative complex. It will therefore altogether elude a psychologist who starts with an attempt to assign separately the simple constituents of the mental life and then, presuming on the adequacy of his analysis, proceeds to account for conation as a mere combination of the only 'simples' which he is prepared to recognise as such. But this method, more or less consistently pursued, is at present the prevalent one among experimental psychologists. It is described by Titchener as "beginning with the simplest and working up to the most complex" instead of "beginning with the 'real mind' and travelling in this way from the better known to the less known[1]." This mode of procedure may have much to recommend it where we are dealing with 'simples' which can be separately attended to; but it is obviously futile where we are dealing with 'simples' which elude inquiry altogether when the inquiry fails to keep in view the unity of the whole to which they belong as inseparable features or aspects. The natural result is the result which has actually been reached by most laboratory specialists. The only primary constituents recognised as entering into the constitution of complex mental processes are sensations. Titchener, for example, takes it for granted that "to begin with the simplest" is "to begin with sensation." "Ought one," he asks, "to begin with the simplest and work up to the most complex—to begin with sensation and to end with mind?." From this point of view the nature of cognition as well as conation is falsified and even the 'affective qualities' are either resolved into sensation, or, if their distinct existence is admitted at all, it is admitted in a vague and dubious way. The assumption which virtually controls most work in psychological laboratories is that the 'real mind' is merely a sensation-complex. As a consequence the point of view of psychology tends to become identified with an extravagant form of subjective idealism. For sensations are modifications of the immediate experience of the individual, existing only in so far as he actually experiences them at any given moment. Further, they are individual experiences which do not refer to anything beyond themselves as their objects. They may themselves be cognised objects, but they are not themselves cognitive. To speak of the object of a visual sensation is as meaningless as to speak of the object of a potato. In the mere sensing of a sensation, abstractly considered, if we are cognitively aware of anything, it is only of the sensation itself as it exists at the moment. Any mental reference to sensations which are not being actually sensed cannot itself be a sensation. Thus, if the 'real mind' is nothing but a sensation-complex, it follows that each of us at any moment can be aware only of his own immediate experience at that moment. Such a position is, of course, totally untenable from the point of view of Theory of Knowledge, and it does extreme violence to common sense. But it may be contended that it is legitimate and useful in psychology. Against this view I feel bound to protest. Our mental life, at any moment, essentially involves mental reference to objects which are no part of our immediate experience at that moment. It is even less possible to treat of it without taking into account this objective reference than it is to treat of eating without reference to food or of walking without reference to the surface walked on or of motion without reference to space. The disastrous consequences of the attempt to carry out this impossible abstraction are especially marked in the case of those modes of psychical existence which are not, like sensations, primarily objective, but are distinctive modes in which consciousness refers to objects[1]. Under this head comes Conation. Conation by its intrinsic nature involves a mental reference to something which is no part of the actual experience of the individual at the moment when he actually experiences the conation. It essentially involves the thought of an 'is to be,' as Mr Bradley would say, in contrast with what actually is. We cannot desire an experience which we actually have in the moment of desiring it; we must, on the contrary, apprehend it as not forming part of the actual situation. Hence any attempt to analyse conative consciousness which ignores objective reference is doomed to failure. Felt tendency, if it exists at all, exists as that mode of immediate experience which gives its distinctive character to the conative reference to an object. Those psychologists, therefore, who systematically disregard objective reference cannot be expected to discover felt tendency. They do not look for it where alone it can be found, and their failure to find it is therefore not a sufficient reason for denying its existence. It is, however, a reason for protesting against the metaphysical prejudice into which they have in part been led by their eagerness to escape metaphysics altogether[2].

---

[1] *Op. cit.*, Vol. II. Pt i. *seq.* Titchener is referring to methods of teaching; but the procedure which he advocates is characteristic of laboratory work in general.

---

[1] Of course I should deny that even sensations could be objective if there were nothing but sensations.

[2] As another example of the subjective idealism of experimental psychologists, I may refer to Titchener's account of Attention. Attention is, according to him, a "state" of consciousness; the word *state* being used as equivalent to the German *Zustand*. It is "the mode or form of existence, which—if we may use the metaphor...the conscious processes of a given time are enjoying.... We speak of attention as 'a state of consciousness' just as we speak of muddiness as a state of the roads, or of a man's affairs being in a bad state. The roads and the affairs are obviously different from the muddiness and the badness,—and that is the difference between the processes attended to or attended from, and attention itself" (*op. cit.* Vol. II. p. 189). Here it is presupposed as a matter of course that what is attended to is always merely the "conscious process of a given time." Apart from this unjustified and unjustifiable assumption, Professor Titchener's position is quite unintelligible. When I attend to another man's arguments or opinions it is nonsense to say that my attention is a state of his argument or opinions as muddiness is a state of the roads. When I attend to the fact that yesterday I had certain sensations my attention is not a mode or form of existence now enjoyed by my past sensations. It would seem that not only conation but cognition evades "introspection under test conditions."

A STUDY OF SOME CORRELATIONS OF THE MÜLLER-LYER VISUAL ILLUSION AND ALLIED PHENOMENA.

By W. G. SMITH

Lecturer in Experimental Psychology, University of Liverpool.

Observations obtained by the method of reproduction in regard to normal estimation of a plain line, the effect of the two forms of illusion—magnifying and diminishing, visual and kinaesthetic memory. The formation of two groups—standard and supplementary.

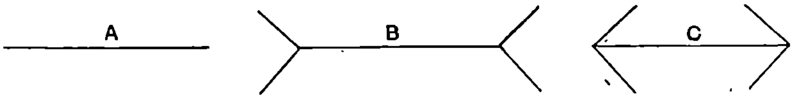
General results of the two groups. The general and individual mean variation. Correlation of normal estimation with the illusions, and other factors. The relations of the two forms of illusion. The relations of visual and kinaesthetic memory. Correlation of the illusions with visual and kinaesthetic memory. The bearing of accuracy in work on the various activities. General comparison of the two groups.

The successive stages in visual and kinaesthetic reproduction. Tendencies to over-estimation and under-estimation in reproduction. The operation of contrast and other factors at successive stages. General conclusions.

In the following pages there are presented the results of a series of tests, which were made with a large number of students, regarding the well-known 'feather' and 'arrow-head' illusions. The aim has been to inquire by means of the statistical method of procedure whether any phenomena can be discovered which will throw fresh light on the meaning and relations of these illusions. At the same time use has been made of the material thus gathered together for the elucidation of other points of interest which have come under consideration during the course of working out the results.

The observations on which this report is based were made by students of the Warrington Training College for Women, who were in the second year of their course. I am greatly indebted to Canon Morley Stevenson, Principal of the College, for permission to utilise the work of the students in this paper. The work was carried out in connection with a course of University Extension Lectures on Physiological Psychology and formed part of the exercises which accompanied the course. The purpose of these observations was to initiate the members of the class into some of the methods of experimental procedure, and to draw attention to some of the phenomena of illusions and of simple sensory memory. It will be easily understood that in such exercises one has to give instructions and choose methods of procedure which are simple and are likely to be clearly grasped and kept in mind. At the same time the mass of the observations thus secured, the uniformity of the conditions of work, and the care and interest with which in general the work was done, seemed to merit close examination and a permanent record of the results.

The observations were carried out on the following plan. The students were requested first of all to trace exactly in ink three pattern lines of 10 cm. in length, each line being traced on a separate sheet of paper. One of these lines, which will be termed the plain line *A*, was left untouched; to the second line, *B*, were attached accessory lines making an angle of 135° with the chief line, *i.e.*, 45° with its continuation; to the third line, *C*, were attached lines making an angle of 45° with the chief line. In each case the accessory lines were to be one inch—25·4 mm.—in length, *i.e.*, approximately a quarter of the length of the main line.



The lines were then copied in pencil, in regular order, on another sheet of paper. First there were drawn three lines[1], which in the judgment of the subject were equal in length to the line *A*. Next the sheet containing the *B* line was brought forward, and again three lines were drawn corresponding to the apparent length of this pattern line. In the third place the *C* line was copied in the same way. In drawing lines equal to the *B* and *C* pattern lines no attempt was made to copy the accessory lines; attention was concentrated on the apparent length of the main line. By this means a basis was secured for comparing the subject's normal perceptual estimation of a line with the estimation when modified by the factors operative in the Müller-Lyer illusion. These determinations were then repeated as before, with this difference that the lines were taken up in the order *A*, *C*, *B*, instead of the previous order *A*, *B*, *C*. The sheet containing the pattern line was in each case laid at the left hand, beside the sheet on which the copies were being made, and was visible as long as the lines were being drawn. Each reproduction was covered by a sheet of paper as soon as it was finished, so that in the case of each new line we have a fresh determination of the apparent magnitude of the pattern line. By variation of the order in which the determinations were made any possible influences due to practice, contrast, or other factors, were largely neutralised.

---

[1] In the quantitative determinations of the illusion reported by Müller-Lyer in his original paper the length of the lines was indicated by two pencil marks on a sheet of paper. Du Bois-Reymond's *Archiv f. Physiol.* 1889, Suppl. Bd., S. 263.

On completion of these experiments the student took up again the plain line *A*, looked at it so as to get a clear idea of its length, and then, laying it out of sight, drew in succession ten lines, each line, as before, being covered up when completed. In this way data for estimating the character of the subject's visual memory were obtained. In the final set of tests the subject was requested to begin by following the *A* line exactly with the pencil, the fingers being placed near the pencil point; the eyes were open, but attention was to be directed as far as possible to the movements of the hand. Then with eyes shut the subject drew ten lines equal to the pattern line, a number being attached to each line as soon as it was completed, so that the order of succession of the reproductions might be clearly recognised when the lines were subsequently examined. In this last set of tests the process of kinaesthetic memory, though by no means studied in an isolated form, is yet much more prominent than in the preceding set of experiments: we shall accordingly in this case speak of the resulting values as determinations of kinaesthetic memory. A perfectly satisfactory determination would have involved a much more complicated procedure, which in the circumstances was not practicable.

After an interval, which varied with the different students, but was on the average about five minutes in length, the experiments dealing with the plain line and the illusions were repeated in precisely the same form. There were thus secured from each student twelve determinations of the apparent magnitude of each of the three lines, *A*, *B*, and *C*, and ten determinations of the visual and the kinaesthetic modes of reproduction. It should be mentioned that before carrying out these experiments the students had already on a previous occasion gained experience and facility in dealing with similar observations. Instructions giving the details of procedure were dictated at a previous lecture, and the work, which was carried out by the students assembled in a large class-room, was carefully supervised by a member of the College staff. Both the sheets containing the pattern lines and those containing the copies drawn by the students, together with their descriptions of the method of work, were handed to the lecturer. In this way it was possible to form a definite conclusion as to the completeness and accuracy with which the various requirements had been carried out. In general the papers reached a high level of care and neatness in execution.

The lines were measured to fractions of a millimetre, the fractions employed being 0·3, 0·5, 0·7; a more minute estimation did not appear to be either necessary or useful. A lens was used to make out the exact dimensions of the line where the termination of the pencilled line was indistinct. The deviations of the pattern lines from the standard length (100 mm.) were in all cases noted and employed subsequently in adjusting the various results to a common standard. Thus, for example, if the pattern line traced by the student was in reality 100·5 mm., then the average length of the copies from this pattern line was reduced by 0·5 mm. Out of the sets of observations handed in about a third were, for the purposes of this report, excluded as presenting too inexact figures, or as not conforming with sufficient closeness to one or other of the conditions which had been laid down. Among those which remained, fifty in number, there were differences to be observed in the precision and neatness with which the pattern lines *B* and *C*, embodying the illusions, had been traced. It seemed unwise to restrict the scope of the report by excluding altogether the less careful papers; on the other hand they stood on a different footing from the other more precisely executed drawings. The treatment which in the end was judged to suit the case best lay in forming two groups. One of these, which will be termed the *standard* group, included the students, 26 in number, whose drawings were more perfectly executed, while the other, to be termed the *supplementary* group, containing 24 students, included those whose drawings were less perfect. It should be remarked that this grouping was decided on after I had completed all the measurements; in this way an equally careful treatment of all the work was guaranteed. The division of the subjects into the groups was made without regard to the magnitude of the illusion or any other factor than that indicated above[1]. While the division into groups has little or no significance in regard to some of the special questions which are raised later on, it has this general advantage that it gives us a means of observing the effect of roughly fractioning the observations.

It will be noted that in what follows the main stress, in the discussion of results, is laid on the determinations of the standard group: in accordance with this treatment the mathematical analysis is in general less elaborate where the results of the supplementary group are concerned. This latter group may be regarded as affording an answer to the question, whether the hypotheses suggested in considering the values of the standard group can be verified in a group offering a somewhat less careful series of observations. Where the indications of the two groups correspond we may be fairly certain that we are on the traces of a general law; where, on the other hand, the two groups give completely contradictory answers, the questions involved should in general be left open. It should be borne in mind throughout that if we had not possessed in the students' tracings of the pattern lines a criterion for distinguishing between the reagents, there would have been no reason for attaching greater weight to one set of observations than to another. As the matter stands, however, we know from the start which observations are more reliable, and we are able to observe the bearing of the distinction, not merely on the magnitude of the illusion and other factors, but on internal criteria of trustworthiness, such as the mean variation.

The values for the various groups and columns are in every case based on the arithmetical mean of the results. This mean in the case of each individual is based, as will be remembered, on 12 determinations for each of the lines *A*, *B*, *C*, and on 10 determinations for visual and kinaesthetic memory. In this way the final or general averages for the

---

[1] The only exception to this statement is found in the case of one subject, who at first was included in the standard group, but in the end was entirely excluded, inasmuch as her average values differed extremely, not merely from the averages of that group, but from those of any member of the group. The figures are as follows (cf. Table I.)—

A	*B*	*C*	Vis. Mem.	Kin. Mem.
125·0	125·8	124·2	181·2	103·0

The admission of such results would have gravely injured the representative character of the averages. Such a rule as that of Holman (quoted by Bliss in his paper "Investigations in Reaction-time and Attention," *Yale Psychol. Studies*, 1898) amply justifies the exclusion:—"Take the mean and the average deviation of the observations, omitting the doubtful one. Then reject the observation if its deviation is greater than four times the average deviation."

standard group (26 subjects) are based on 312 separate values for the lines *A*, *B*, *C*, and on 260 values for visual and kinaesthetic memories. For the supplementary group (24 subjects) the corresponding numbers are 288 and 240.

General Results.

The first values which fall to be presented (Table I) are those which bring out the general features of the lines *A*, *B*, *C*, as drawn by the subjects, and of the two forms of memory (*Vis. Mem.*; *Kin. Mem.*), as well as the relations of the two groups (*Stand.*; *Suppl.*). There appear also in the table, under the headings *Magn. Ill.* (Magnifying Illusion) and *Dim. Ill.* (Diminishing Illusion), the figures which indicate the average magnitude of the illusion in line *B*, where the line is lengthened, and line *C*, where the line is shortened. These figures are obtained by finding the difference between the value of the line *A* and those of the lines *B* and *C*; the assumption being made that we have in the values for the plain line *A* a reliable indication of the subject's normal estimation, and that the effect of the illusion must be gauged by reference to this normal estimation.

TABLE I. *General Results.*

	A	*B*	Magn. Ill.	*C*	Dim. Ill.	Vis. Mem.	Kin. Mem.
Stand....	98·9	104·6	5·7	94·4	4·5	100·9	85·3
Suppl....	100·0	106·4	6·4	95·2	4·8	103·5	84·5

It will be observed that the two groups are in close agreement in regard to the main features of the different columns. At the same time the values given by the supplementary are higher than those of the other group in each column except the last. The former circumstance is of considerable importance in considering the question as to the weight which should be attached to the results of the supplementary group. If the ratio in which the groups stand in regard to the lines *B* and *C* had been found to hold only in these lines, we should have been obliged to suppose that the characteristics shown in the less perfect drawing of the lines *B* and *C* had reference only to these two lines. As it is, we see that these results for *B* and *C* are not isolated, but fall into line as instances of a general tendency exhibited in an equally

n16287

strong degree in the process of copying the plain line *A* and in visual memory. Other facts of a similar character will be noted later on. Taking the magnifying and diminishing forms of the illusion together we have as the value of the combined illusion, 10·2 mm. in the standard, and 11·2 mm. in the supplementary group.

As the methods of work and dimensions of figures differ considerably in the various quantitative investigations which have been published on this subject, there is little basis for instituting a comparison in regard to the magnitude of the illusion. This is of comparatively little importance inasmuch as the interest of the present investigation lies, not in absolute values, but in the relations of the various processes involved in the different tests. It may, however, be pointed out that certain data presented by Heymans[1] are in fairly close agreement with those given here. As the result of observations from 25 persons, with a figure in which the main line was 75 mm. in length, and the angles made by the accessory lines had a magnitude of 50°, it was found that the combined illusion amounted to 10·8 mm. when the accessory lines were 20 mm. in length, to 13·0 mm. with accessory lines of 30 mm. and to 12·6 mm. with accessory lines of 40 mm.[2] This would indicate that in Heymans' subjects the illusion was somewhat more effective. Such a fact is easily understood when it is remembered that in the present investigation the subjects constructed their own figures and were thus aware from the start that the various pattern lines were objectively equal. The magnifying form of the illusion is seen to be decidedly more effective in both groups than the diminishing form: this result is in agreement with the conclusions of Heymans[3] and Binet[4], though the effectiveness of the magnifying form is by no means so striking here as it appears in the papers of these authors. With the former the magnifying form of the illusion is about twice as large as the diminishing form; with the latter it is about four times as large. Differences in method and in the character of figures no doubt contribute, together with the circumstance mentioned above, to explain this difference. Out of the 50 individuals tested in the present investigation 3 individuals, *i.e.* 6 %, were found not to be subject to the magnifying illusion, while in

---

[1] *Zeitschr. f. Psychol. u. Physiol. d. Sinn.* 1895, IX. S. 229. In this investigation the procedure consisted in adjusting a framework of cardboard, so as to secure apparent equality of the lines embodying the illusion.

[2] *Loc. cit.*, Table V, p. 229.

[3] *Loc. cit.*, S. 234.

[4] *Rev. Philos.* 1895, XL. p. 11. In this investigation the method of selection was employed, the school children, with whom the experiments were made, choosing out of a series of different lines the one which appeared equal to another already presented.

6 individuals, *i.e.* 12 °/₀, there was no trace of the diminishing form. This result also is in conformity with the experience of Binet. It is thus seen that the diminishing form of illusion is not merely smaller in magnitude than the magnifying form; it is also less prevalent. That the smaller magnitude of the diminishing form is not due to the inclusion in the general averages of the results from the 6 subjects who show no trace of its incidence, has been demonstrated by a special calculation.

Mean Variation and Probable Error.

The mean variation is presented in two forms, general and individual. By the general, or statistical, mean variation (M.V.) is meant the average deviation of the individual means from the general mean, or average of the group. By the individual mean variation (*m.v.*) is meant the extent to which each reagent deviates from her own mean: when this has been determined for each individual we can then find the average of these individual mean variations for each group. For the standard group the probable error has been calculated according to the formula,

$$\text{pr. err.} = 0{\cdot}6745\sqrt{\frac{\Sigma v^2}{n(n-1)}};$$

for the supplementary group the less detailed treatment expressed in the formula,

$$\text{pr. err.} = 0{\cdot}8453\,\frac{\Sigma v}{n\sqrt{n-1}},$$

has been adopted.

TABLE II. *General Mean Variation and Probable Error.*

	A		*B*		Magn. Ill.	*C*		Dim. Ill.	Vis. Mem.		Kin. Mem.	
	M.V.	pr.err.	M.V.	pr.err.	pr.err.	M.V.	pr.err.	pr.err.	M.V.	pr.err.	M.V.	pr.err.
Stand.	3·9	0·64	3·7	0·60	0·55	3·9	0·69	0·52	6·5	1·09	8·5	1·37
Suppl.	4·2	0·74	4·8	0·84	0·64	5·8	1·02	0·57	6·3	1·11	10·3	1·32

With these data in hand we can observe how the results of the present research stand related to those of other investigators. The probable error of the three values for the combined illusion which have already been cited from the paper of Heymans are, respectively, 0·61, 0·81, 0·90; when the action of the two forms of illusion was separately investigated the values ranged from 0·68 to 0·75. In experiments by Einthoven[1] regarding the effect of horizontal and vertical position on the magnitude of the illusion the probable error ranges from 0·62 to 1·06 for a group of 11 subjects. Binet presents the mean variation in place of the probable error: with 60 pupils of the average of 12 years the extent of the mean variation with a line of 100 mm. was 7·5 mm. in one series of determinations and 9·6 mm. in another. In the experiments reported by Rivers[2] the average mean variation (M. V.) for 70 English subjects, men, women and children, ranges from 3·27 to 4·49, with a main line of 75 mm.

It will be observed, on reference to Table II, that in each column the probable error of the supplementary group is somewhat larger than that of the standard group; the same fact is brought out with one exception (Vis. Mem.) by the mean variation. The conclusion seems clear that the supplementary group is composed of individuals who are less homogeneous, less uniform in character than those in the other group. It is interesting to notice that while the homogeneity in the results of the standard group is not appreciably altered by the action of either form of illusion, the opposite is true of the supplementary group. In other words, it may be said that where the variability is relatively high, as we have seen it to be in the supplementary group, the action of the two forms of illusion is in the direction of heightening this tendency. While the results for the *B* line are more homogeneous than those for the *C* line in both groups, the opposite holds in regard to the corresponding forms of illusion. What the explanation of this fact may be has not become clear.

Binet found, on comparing individual estimates of the length of a plain line, that "les differences individuelles s'attenuent presque au point de disparaître[3]." So far from such a relation holding here, we find that in the standard group the individual differences are in fact greater with regard to the plain line than they are with regard to the *B* line. A basis of comparison with other results may be found in certain experiments of Rivers[4] in which one line, horizontal or vertical, had to be adjusted so as to be equal to another line, vertical or horizontal, of 100 mm. in length. The general mean variation (M. V.) of a group of 50 women (students in a training college) in these experiments ranged from 2·7 mm. to 3·5 mm. Reference may also be made to the values in the first and third columns of Table IV; the general mean variation in these columns is practically identical with that in the corresponding column (A) in Table II.

---

[1] *Pflüger's Archiv f. d. ges. Physiol.* 1898, LXXI. S. 27. The method in these experiments was similar to that of Heymans.

[2] *Brit. Journ. Psych.* 1905, I. p. 357.

[3] *Loc. cit.*, p. 23.

[4] *Loc. cit.*, p. 354.

TABLE III. *Individual Mean Variation.*

	A	*B*	*C*	Vis. Mem.	Kin. Mem.
Stand....	3·3	3·5	3·6	2·8	5·3
Suppl....	3·8	4·2	3·7	3·0	5·4

It is interesting to find from Table III that, though the principle on which the groups were formed, viz. excellence in drawing, has no direct relation to *individual* variability, the supplementary group is throughout less regular and uniform in its work than the standard group. There appears thus to be a slight, but persistent, correlation between the homogeneity of a group and the homogeneity of the work done by the individual members of the group. It may be pointed out, in support of this generalisation, that, when the appropriate calculations are made, the data regarding estimation of lines reported by Rivers[1] show that in the varied groups with which his work deals the individual mean variation is large in those cases in which the general mean variation is large. It is to be noted also in the data before us that those subjects whose work is irregular in one test, exhibit irregular work in all the other tests.

The fact that the figures for individual variability in visual memory are smaller than those for copying the plain line in both groups requires further consideration. It would appear to suggest that the psychophysical processes are more constant in character when the forces concerned are predominantly central, than they are when under constant control through the presence of an objective stimulus, viz. the continuously visible pattern line. Inasmuch, however, as the series of 12 determinations of the plain line was interrupted by the interval, and by the alternate direction of attention to the other lines *B* and *C*, it seemed necessary to make this point the subject of special inquiry. With this aim in view observations were secured from 10 persons, none of whom is included in the other groups: the method of work followed as closely as possible that in the experiments already described, except that in each case the series of determinations was continuous. A line of 100 mm. was presented to each individual and he was asked to draw in succession 10 lines, (1) with the pattern line visible all the time, *i.e.* by normal perceptual estimation, (2) from visual memory, (3) again with the pattern line visible, and finally, (4) from visual memory. The fact that there are comparatively few subjects is compensated for by the larger number of determinations secured from each, viz. 40 lines. In the following table the values for each series are given separately, the numbers attached to each heading indicating the order in which the experiments were made.

---

[1] *Loc. cit.*, pp. 348, 354.

TABLE IV. *Comparison of Normal Estimation with Visual Memory.*

	1. Normal Estimation	2. Vis. Mem.	3. Normal Estimation	4. Vis. Mem.
Averages	101·6	104·9	103·8	105·2
General mean variation	4·0	4·6	4·0	5·6
Individual mean var^{n}.	2·6	2·9	2·7	2·9

The fact that the individual mean variation for visual memory in this table is practically identical in both columns with that for the two groups in Table III gives us good ground for concluding that the divergence in the corresponding values for normal estimation is due to the fact that the determinations of the *A* line in the original experiments were not carried out continuously. As a corollary of this it may be concluded that the values in the second and third columns, *B* and *C* of Table III, are likewise higher than they would be with continuous determinations. As however the same conditions apply to all three, their relative magnitudes may still be assumed to be correctly expressed. The general mean variation in columns 1 and 3 of Table IV is practically identical with that in column *A*, Table II, while the values for visual memory in Table IV are somewhat lower than in Table II.

Taking the present table, then, as giving us a satisfactory basis for comparing the regularity of individual work in perception and memory, we see that the relation suggested by the results of the former table does not hold, and that the complex of processes involved in visual memory is distinctly less constant in its operation than that involved in perceptual copying. We observe also that the connexion of individual and general mean variation shows itself here again in the fact that where the one is higher, viz. in columns 2 and 4, there the other is higher also.

Correlation of Normal Estimation with the Illusions, and other Factors.

Before the special problems of correlation are considered, the general methods utilised in determining its presence may be briefly referred to. The first and simplest treatment, which will be termed the method of grouping, has been applied to all the results. It consists in dividing the members of each group into two halves, or sub-groups, on the basis of the magnitude of any factor we are considering, *e.g.* the length of the line drawn in normal estimation, when the subject is copying the plain line *A*. In one sub-group are placed those subjects whose lines are longer, in the other those whose lines are shorter. The next step consists in finding for each sub-group the magnitude of any other factor, *e.g.* visual memory, whose relation to the first is being examined. Should the sub-group, whose drawing of the line in normal estimation is relatively long, show at the same time a relatively long line in visual memory, then we have evidence, more or less strong, of direct correlation.

The second, more detailed method, which has been in general applied to the results of the standard group alone, consists in the application of Pearson's formula for determining the degree of correlation of two series[1],

viz. $$\frac{\Sigma xy}{n\sigma_1\sigma_2},$$

where x and y represent the variations from the arithmetical mean of each series, and σ_1, σ_2 represent the standard deviation, or mean square error, of each series. In utilising this method it is to be remembered that complete direct correlation is indicated by unity, while zero represents absence of correlation, and negative values indicate inverse correlation in which one factor increases as the other diminishes.

In the third method, which may be termed the method of percentages, the individuals in each group are arranged, as in the first mode of procedure, in two equal sub-groups, one of which contains those whose values are above the average, say, in normal estimation, while the other contains the remaining members. It is then noted in how many instances the individuals who are above the average in normal estimation are also above the average in regard, say, to visual memory, and also how often those below the average in the one case are at the same time below the average in the other. There is evidence of a very high degree of correlation when the percentage of cases of agreement in regard to being both above or below the average reaches about 100 °/₀; when it falls to 50 °/₀ there is no evidence of correlation.

---

[1] *The Grammar of Science*, 2nd ed., p. 400.

The table which follows has been constructed on the basis of the method of grouping in order to answer the question, how far the tendencies present in normal estimation are operative in the other activities which have been tested. The upper horizontal line for each group gives the average values attained by the sub-group whose lines are made too short in copying the plain line *A*: the lower horizontal line gives corresponding values for the sub-group which over-estimates the *A* line.

TABLE V. *Correlation of Normal Estimation with Illusions and other Factors.*

		A	*B*	Magn. Ill.	*C*	Dim. Ill.	Vis. Mem.	Kin. Mem.
Stand.	1. Short *A* Line	95·0	102·6	7·6	91·2	3·7	96·4	78·6
	2. Long *A* Line	102·8	106·6	3·8	97·6	5·2	105·3	92·0
Suppl.	3. Short *A* Line	95·8	103·4	7·6	91·0	4·8	99·0	81·9
	4. Long *A* Line	104·1	109·4	5·2	99·3	4·8	108·0	87·0

It is of course clear that we have not got here, or in the other tables, a pure measure of perceptual estimation, such as we should get, say, by the method of right and wrong cases. The estimation of length is complicated by the movement involved in constructing the line. But the ambiguity is one which is inherent in many applications of the method of reproduction, or average error; the main difference in the present case is that we have adjustment by means of pencil and not by special apparatus. Data for gauging the influence of these elements are not available. It may, however, be assumed with considerable probability that the activity of construction mainly accentuates individual tendencies and attitudes: this is of special value in the present investigation, in that it assists in the analysis of the various forces which are operative in the results. The fact that this method of drawing lines involves a continuously *increasing* series of magnitudes as the pencil moves along, probably introduces a slight error in the direction of shortening, owing to the operation of expectation. Inasmuch as this error is a constant feature in all the processes which are being studied here, it is of little or no significance for our purposes. The error due to having the pattern line always on the left hand may also be neglected.

The most obvious point in the table is that the tendencies to over- and under-estimation of the *A* line show themselves with great distinctness operative in each of the other main columns. In both groups, standard and supplementary, the individuals whose lines are too long when they copy the *A* line, draw lines which are too long where other activities are concerned; the same thing holds in the case of lines that are too short. In other words, we have, by classifying the subjects on the basis of the normal estimation, secured evidence of definite and persistent tendencies, which may be regarded as fundamental individual characteristics. It is to be noted also in this connexion that the tendency in the supplementary group to the drawing of relatively long lines, reappears in each of the sub-groups of this table, with one exception, viz. col. *C*; the supplementary sub-group in general draws a longer line than the corresponding standard sub-group.

Similar conclusions appear justified when the method of grouping is applied to the results embodied in Table IV. If we take the five individuals whose values are below the general average for normal estimation and find their averages for this process and for visual memory we find that the figures for the two processes respectively are 99·0 mm. and 100·8 mm. For those whose values are above the average in regard to perception the corresponding figures are 106·4 mm. and 109·3 mm.

To this evidence for the operation of persistent tendencies another fact may be added. When the individual is faced by the initial simple problem of measuring off with the scale a pattern line of 100 mm.—the plain line *A*—the standard group makes it on the average 0·11 mm. too long; on the other hand, it is too long by 0·23 mm. in the other group. And the general ratio holding between the groups, holds also between the subdivisions of the groups already described. In the first sub-group (stand.) the pattern line is too short by 0·07 mm., while in the second it is 0·28 mm. too long; in the third sub-group (suppl.) it is too long by 0·18 mm., while in the last it is too long by a larger amount, viz. 0·28 mm. In this way we observe how the individual bias modifies the effort made under the conscious intention of accuracy, and with every opportunity for perceptual correction.

Turning now to the connexion between the values in the first column and those for the two forms of illusion, we remark that in both groups the magnifying illusion is greatest with the sub-group which in normal estimation draws the shorter line, and is least where the normal estimation involves over-estimation of the plain line. Again, the diminishing illusion is least in the sub-group which under-estimates in the case of the standard group, while in the other group there appears no indication of any kind of correlation. Analysing the results of the standard group by the detailed method we find the correlation between the length of the *A* line and the magnifying illusion to be − 0·41, while that between the *A* line and the diminishing illusion is + 0·31. In view of the importance of the points at issue here, this method has been applied to the supplementary group also, with the result that for the *A* line and the magnifying illusion the correlation is − 0·33, while that for the *A* line and the diminishing illusion is − 0·095. The results of two methods are in conformity throughout, for the value − 0·095 cannot be regarded as indicating a noticeable degree of correlation. Neglecting for the moment this exceptional value, we may sum up these results in the statement that there is negative or inverse correlation between normal estimation and the magnifying illusion, and positive direct correlation between normal estimation and the diminishing illusion: over-estimation of the plain line goes with a large diminishing illusion and a small magnifying illusion, while with under-estimation the reverse relation holds.

At the first glance these values appear to possess little significance. On further consideration it becomes clear that the various data can be combined without difficulty in the statement that each form of the illusion tends to reach its maximum effect where the preexisting tendency, apparent in normal estimation, is in the opposite direction. It may be that this principle has a scope wider than the operation of illusions and extending to the operation of other factors which modify perceptual magnitudes; all that is contended for here is the antagonism between preexisting tendencies to increase or decrease and the operation of illusions so far as they are magnifying or diminishing. It would appear that the psychophysical principle at work here is analogous to that embodied in Weber's Law. With the operation of the tendency to over-estimation the effect of an agency making for increase in length is lessened; with a stronger tendency to under-estimation the effectiveness of the diminishing illusion is lessened.

The Relations of the Two Forms of Illusion.

The next question to which an answer may be sought in these data concerns the connexion of the two forms of illusion with each other. It is a question which does not seem to have been raised; but when once raised it is seen to be of some interest and importance. As before, the first method to be applied is that of grouping. It is found then that the section or sub-group in the standard group which exhibits the larger magnifying illusion has an average value for the diminishing illusion of 3·8 mm., while the section in which the magnifying illusion is less strong has an average value for the diminishing illusion of 5·1 mm. Worked out by the detailed method the degree of this inverse correlation is found to be $-0·26$. The other group, however, does not confirm this conclusion; the section in which the magnifying illusion is most potent gives 5·0 mm. as the value of the diminishing illusion, while for the other section the corresponding value is 4·6: correlation in this case would appear to be either absent, or in a very slight degree direct in character.

In view of these divergent results the only conclusion open to us is to the effect that under the given conditions there is no adequate proof of the general presence of direct correlation. On the other hand, if the standard group were accepted as more representative, it might be contended that inverse correlation, slight in degree, was indicated. The former contention is supported by another line of evidence. It will be recollected that out of the total number of subjects 6 were free from the diminishing form of illusion, while 3 were free from the other form. Taking the former as giving a more reliable basis for analysis, we find that for them the average value of the magnifying form of illusion is 5·9 mm., a figure which stands midway between those of the two groups, viz. 5·7 mm. and 6·4 mm. In other words, the complete ineffectiveness of one form of illusion would appear to give no ground for expecting either heightened or diminished susceptibility to the other form of the illusion.

This apparent lack of direct correlation is of considerable interest in view of theoretical explanations of the Müller-Lyer illusion. There has been a strong tendency to treat the two forms of this illusion as standing, for the purposes of explanation, on the same footing, and to suppose that any factor which was regarded as causal in relation to one form, was equally operative in regard to the other. Whether an explanation is sought in impulses to eye movements, as by Wundt[1], or in apperceptive activities, as by Lipps in a recent paper[2], or in lessened visual acuity in the more peripheral portions of the retina, as by Einthoven[3], the underlying assumption, which, indeed, is a perfectly natural one, is that the same explanation serves for both forms of illusion. It is only carrying this assumption to its logical conclusion to expect that an individual, who is highly susceptible to one form of illusion, should in an approximately equal degree be susceptible to the other form—that the correlation should be direct and fairly close. As we have seen, this conclusion does not hold for the comparatively large aggregate of subjects whose results are here analysed. This subject, however, will be referred to again at the close of a later section.

The Relations of Visual and Kinaesthetic Memory.

The application of the method of grouping has already shown, in Tables IV and V, that the tendencies revealing themselves in normal estimation are clearly operative in the processes of reproduction as well. In order to emphasise this and other facts the results will next be analysed by means of the method of percentages. Seeing that none of the topics to be specially discussed in this section have any direct reference to the principle which determined the formation of the groups, the average has been supplied in the lowest horizontal column as well as the separate values for the groups. In the vertical column headed *Vis. Mem., Kin. Mem.*, we have the percentage of agreements in regard to these two activities, *i.e.* the number of instances in which an individual above (or below) the average in respect of one factor is above (or below) in respect of the other: the next two columns are similarly constructed. In the last column are the values for three activities taken together, viz. normal estimation, visual and kinaesthetic memory. It may be added that when the correlation of visual and kinaesthetic memory is determined for the standard group by the detailed method, its degree is indicated by the value + 0·67.

It is noteworthy that the operation of preexisting tendencies, which here again receives ample illustration, is in general less constant and decided in the supplementary group. There would appear to be a somewhat greater correlation of normal estimation with kinaesthetic

[1] *Grundzüge d. physiol. Psychol.* II. S. 149, 4te Aufl.

[2] *Zeitschr. f. Psychol. u. Physiol. d. Sinn.* 1905, XXXVIII. S. 250.

[3] *Loc. cit.*, S. 1.

TABLE VI. *Correlations of Visual and Kinaesthetic Memory: Percentages.*

	Vis. Mem., Kin. Mem.	*A*, Vis. Mem.	*A*, Kin. Mem.	*A*, Vis. Mem., Kin. Mem.
Stand.	78·8	78·8	84·6	71·2
Suppl.	62·5	83·3	62·5	54·2
Average	70·7	81·1	73·6	62·7

than with visual memory in the standard group; on the other hand this relation is reversed in the supplementary group. It may, indeed, be presumed that the low percentages in the first and last columns of the supplementary group are primarily due to a lowered degree of correlation between normal estimation and kinaesthetic reproduction. The general tendency towards correlation of normal estimation and memory has already been noticed by Binet[1]. The normal estimation of a line, as determined both by comparison and by copying, he finds to exhibit fewer errors than memory, but "elles ont lieu dans le même sens."

Correlation of the Illusions with Visual and Kinaesthetic Memory.

The analyses which are now to be presented were undertaken in the hope that additional light might be thrown on the illusions and their modifying conditions. It seemed possible that tendencies which are significant in relation to the illusions might show themselves with some prominence where the predominant processes are those of reproduction. It might also be thought that, since in the two forms of memory we have a decided predominance in the one case of visual, in the other of kinaesthetic processes, the two forms of the illusion might show some specific form of relation to the forms of memory.

In the following tables the results have been analysed by means of the method of grouping with regard to the magnitude of each form of illusion. Table VII presents the analysis from the point of view of the magnifying illusion, while in Table VIII the point of view is that of the diminishing illusion. Two equal sub-groups have been formed in each group: in one of these are the subjects whose illusion—magnifying in Table VII, diminishing in Table VIII—is larger; in the

---

[1] *Rev. Philos.* 1894, xxxvii., p. 349.

TABLE VII. *Correlation of Magnifying Illusion with Memory.*

	Larger Magn. Ill.		Smaller Magn. Ill.	
	Vis. Mem.	Kin. Mem.	Vis. Mem.	Kin. Mem.
Stand..........	100·0	84·5	101·7	86·2
Suppl..........	101·4	84·7	105·6	84·2

TABLE VIII. *Correlation of Diminishing Illusion with Memory.*

	Larger Dim. Ill.		Smaller Dim. Ill.	
	Vis. Mem.	Kin. Mem.	Vis. Mem.	Kin. Mem.
Stand..........	101·9	86·8	99·9	83·8
Suppl..........	101·7	84·6	105·8	84·3

other sub-group, the illusion is smaller in extent. For each sub-group the average values of visual and kinaesthetic memory are then calculated.

The prevailing tendency shown in Table VII is towards inverse correlation between the magnifying illusion and each form of memory. The exception to the rule is found in the values for kinaesthetic memory in the supplementary group. Examined by the detailed method the correlation in the standard group in the case of visual memory is to be expressed by – 0·34, and in the case of kinaesthetic memory by – 0·35. It is of interest to compare these values with the percentages in Table VI, where it is seen that for the standard group the correlation between normal estimation and kinaesthetic memory is not widely different from that between normal estimation and visual memory.

In Table VIII the general rule is that the diminishing illusion is directly correlated with the forms of memory, the larger illusion being found with the longer line, the smaller illusion with the shorter. Here again there is an exception in the supplementary group; in this case it concerns the visual form of memory and is of much greater weight than the previous exception. Measured by the detailed method the correlation in the standard group for visual memory is + 0·31, and for kinaesthetic memory + 0·38.

These various data, which at first are sufficiently puzzling, become more intelligible if we look at them in the light of the conclusions previously reached regarding preexisting tendencies and the consequent correlation. It was found that the two forms of memory were in fairly close correlation, especially in the standard group, and that the forms of memory were in equally close correlation with the normal estimation. On the other hand it was found that each form of illusion tends to reach its maximum where the preexisting tendency is in the opposite direction. If now we apply this latter principle to the relation between the illusions and the forms of memory, we shall readily understand how it comes that there is inverse correlation between the magnifying illusion and both forms of memory. The same explanation serves for the dominant tendency toward direct correlation in Table VIII; here again the illusion is largest, finds its greatest scope, where the preexisting tendency is in the opposite direction. These results, in fact, may be regarded as confirming the validity of this principle.

No completely satisfactory explanation of the exceptions met with in this and in a former section, has come to light. If we make the hypothesis that there is, after all, a tendency to direct correlation of the two forms of illusion, due to their having a common cause or condition, then we must suppose that this tendency will come more or less into conflict with the principle referred to above: in this way the exceptions referred to above might be partly explained. It might then be further supposed that where the illusions were greater in magnitude this common cause or condition would tend to be the prevailing agency. In order to test this last hypothesis there have been chosen from both groups the subjects, 24 in number, whose combined illusion is largest. These again, according to the method of grouping, have been divided into two sub-groups, one containing those whose magnifying illusion is large, the other those with whom it is small. It has thus been found that those with an average magnifying illusion of 12·5 mm. have a diminishing illusion of 5·5 mm., while those with whom the magnifying illusion is only 5·4 mm. have a diminishing illusion of 8·2 mm. That is to say, where the forces tending to the production of illusion are specially strong, there the inverse relation of the two forms is distinctly marked. We have therefore no proof here of direct correlation of the forms of illusion; the hypothesis of absence of correlation finds likewise no support. It is the hypothesis of inverse correlation, already suggested in the previous section, by the data of the standard group, that seems the more satisfactory in view of this last piece of evidence. This view is at the same time the one which would naturally be suggested by the principle governing the relation of the tendencies in normal estimation to the two forms of illusion. As was seen before, this view is exposed to difficulties, but allowance is made for these if we admit that the tendency to inverse correlation is not specially strong, and that its operation may be obscured by the play of accidental variations, such as we have reason to expect in greater abundance in the supplementary group. It is in this group that, according to the conclusions of Tables II and III, there exists greater variability, both in the group as a whole, and in the individual members of the group. The evidence before us does not permit of deciding whether the condition on which the inverse relation is ultimately superimposed is one of direct correlation, or of independent variation of the illusions. The fact that nearly every one is subject to both forms of the illusion is no proof of correlation: it is compatible both with a common origin of the illusions, and with relatively independent causes.

The Bearing of Accuracy in Work on the Various Activities.

The data which will be brought forward in this section form an answer to the question:—does the work of those subjects whose normal estimation is characterised by greater accuracy present any special, marked features when the results of the other tests are considered? The answer has been sought exclusively through the use of the method of grouping. It has been assumed—and the assumption does not appear open to reasonable objection—that a trustworthy criterion of accuracy is to be found in the closeness with which the copies drawn by the subjects in the process of normal estimation approach the length of the standard line. Those individuals, 12 in each group, whose drawing of the plain *A* line deviated least on the average from 100 mm., were formed into 'accurate' sub-groups: it was then determined for each sub-group what was the average in regard to each of the other points investigated. In the following table the upper horizontal line for each group repeats for the most part values for the whole group which have been already presented: in this way comparison is facilitated with the new values in the lower line for the sub-group of accurate workers. In the last vertical column is given the average of the five values for individual mean variation given in Table III.

TABLE IX. *Characteristics of Accurate Workers.*

		A	*B*	Magn. Ill.	*C*	Dim. Ill.	Vis. Mem.	Kin. Mem.	Ind. m.v., *A* Line	Ind. m.v.; av. 5 values
Stand.	Gen. Av.	98·9	104·6	5·7	94·4	4·5	100·9	85·3	3·3	3·7
	Accurate	99·8	105·2	5·4	95·7	4·1	100·5	87·9	3·3	3·7
Suppl.	Gen. Av.	100·0	106·4	6·4	95·2	4·8	103·5	84·5	3·8	4·0
	Accurate	99·3	106·1	6·8	93·8	5·5	102·6	83·5	3·9	4·0

It must be admitted, if we give equal weight to the results of both groups in this table, that the accurate workers do not show themselves superior to, or different from, the general body of reagents. Both forms of illusion are less with accurate workers in the standard, but both are greater in the supplementary group. The plain line and the kinaesthetic reproduction approach nearer to the standard line in the case of the accurate workers of the standard group: they depart further from it in the supplementary group. In visual memory alone do the two sub-groups show an agreement in increased accuracy. The practical identity of the values for main and sub-groups in the last two columns is a noteworthy feature: it indicates a great degree of similarity within each group. The paradox of the supplementary group—that the accurate workers in regard to the *A* line are the less accurate—is of course due to the accidental neutralisation, or balancing, of all the errors in the general average.

Auerbach, following out his theory that the Müller-Lyer illusion is due to our taking into account, not merely the main lines, but also imaginary lines which might be drawn parallel to the main line, makes the suggestion that individuals who are accustomed to concentrate their vision on definite objects will be as a rule less liable to the illusion. "Das letztere gilt in besonders hohem Masse von den Vertretern gewisser Berufe, z. B. von den Jägern und den Mathematikern, und in der That stellt sich in Beispielen dieser Art der Fehler als ungewöhnlich klein heraus[1]." It seems permissible to apply the figures given above as a test of this hypothesis: those subjects whose copying of the *A* line is most exact may be assumed to be the most definite and reliable in visual perception. As has been seen above the accurate workers cannot claim any superiority when both groups are equally considered. If, however, we accept the standard group as more representative and reliable, then we have evidence in support of Auerbach's contention. We have, it would seem, the right to accept it on two grounds. There is first the general fact that the standard group has already been found to be more regular, less open to the play of accidental variation. There is, secondly, the special circumstance that, while the accurate workers in the standard group show one tendency throughout, the corresponding supplementary sub-group is not consistent; certain of its indications tending in one direction, others in another. The problem of explaining the divergent values of the supplementary group would, however, still remain.

---

[1] *Zeitschr. f. Psychol. u. Physiol. d. Sinn.* 1894, VII. S. 155.

Before commenting on certain other features of the results I should like to emphasise the distinction between two possible sources of error and inaccuracy. The one lies in defective care and attention; this, it is obvious, is a general psychophysical cause or condition, operative here only because the reagent is engaged in drawing lines. The other lies in the special character of the visual sensory mechanism. The former will characteristically be measured by the individual mean variation; it may also be operative to some extent in the constant error of over- and under-estimation. The sensory factor will be more nearly indicated by the constant error of the individual or the group; this will not necessarily be affected by the individual mean variation, though of course sensory defect may be accompanied by defective attention.

The homogeneity of the standard group was referred to in one of the earlier sections as being evidenced by the relatively low general mean variation. It has been emphasised again by the identity of the individual mean variations for the whole group and for the section of accurate workers. This makes it clear that the sections in this group are not differentiated in respect of the character of the attention exercised. On the other hand there is in the accurate section a moderate, but regular, decrease in the constant error; this we may fairly attribute to change in the special sensory processes. The supplementary group on the whole differs from the other in possessing a greater individual mean variation and a greater constant error: it would thus appear to be qualitatively inferior, both in regard to attention and to sensory accuracy. Within the supplementary group the similarity of the members in regard to attention appears to be as great as in the former group: the accurate section possesses the same character in regard to variability as the whole group. The varying direction of the constant errors in the accurate section of this group makes attempts at explanation difficult: perhaps the best procedure is to regard them as products of the relatively high variability which characterises the group as a whole. In a former paper[1] there was reported a series of experiments regarding the length of lines, carried out by epileptic and normal subjects, which touch on the present discussion. It was there pointed out that in the section of subjects in which sensory discrimination, as measured by the number of correct judgments, was greater, the individual mean variation was distinctly decreased. We may suppose that in that section there was improvement at the same time in the two factors of attention and sensory accuracy. And we have good grounds for finding a parallel between the relations of these sections in the former investigation and those of the two main groups in the present research.

General Comparison of Standard and Supplementary Groups.

We are now in a position to sum up the main characteristics of the two groups whose detailed results have been studied. The supplementary group draws a longer line than the standard group in each of the activities we have studied, with the exception of kinaesthetic memory: this result, shown in main outline in Table I, is confirmed in detail in Table V. This tendency towards over-estimation, or exaggeration, appears again in the two forms of illusion when they are isolated; the magnifying and diminishing forms are both greater in magnitude in the supplementary group. We have seen a similar tendency at work in the measuring of the pattern line *A*. When the variability of the groups is studied with the help of mean variation and probable error, it is found that, with one exception, the supplementary group is characterised by a larger general range of variation. A larger individual mean variation, again, is characteristic throughout of the work done by members of the supplementary group. There is in the supplementary group a lessened tendency to correlation of the various activities which have been studied, as is shown in Table VI and elsewhere. We are no doubt justified in connecting this last feature with the fact that, where exceptions to the various general tendencies or rules exist, they have been found exclusively in the results of the supplementary group. It is difficult to focus all these characteristics in one statement, but it may perhaps be said that inability to draw with adequate precision and care a simple geometrical figure is correlated with a general increase of variability and incoordination.

---

[1] *Brit. Journ. Psych.* 1905, I. p. 251.

The question may be raised here whether the differences between the various groups and series of values are in all cases really significant. It has not seemed necessary to calculate the probable error of the average for every fresh grouping or series. But there does not seem any good reason for supposing that the probable error of the various sub-groups would in any case materially exceed that of the whole group. And, using as a general criterion the ratio of the magnitude of any difference which is in question to the probable error of the series concerned, we shall find that seldom, if at all, has a difference between sub-groups or series been emphasised which does not more or less exceed the magnitude of the probable error. In addition to this circumstance we have in several cases the additional support of converging methods of analysis and of a consideration of the place which any individual result occupies in the general scheme of values for each group and for both the groups.

Now that the work of the students has been examined and analysed it is possible to see how the advantages of greater insight, gained by the formation of the groups, could be gained without any uncertainty as to the effect of the less perfect figures on the subjects' estimations. We might have figures of the illusions printed on separate sheets and distributed to each person undergoing the tests. At the same time each person might be requested, at some time during the course of the experiments, to prepare careful drawings of the two forms of the illusion, or of some simple geometrical figure. In this way we could be certain as to the character of the lines which the subject had before him, and we should still be able to see the relation of varying care and skill in drawing to the various features in the results. Other useful modifications might be suggested for work designed from the start to supply material for scientific analysis[1].

The Successive Stages in Visual and Kinaesthetic Reproduction.

At the outset the data were analysed with the view of finding whether characteristic curves could be found, not merely for those subjects whose lines tended to increase or to decrease during the course of the 10 reproductions, but also for those with whom these tendencies in either direction were relatively slight. As, however, this analysis did not bring anything noteworthy to light it seemed best to confine the investigation to the two sections first named, and it is from this point of view that the following table and curves have been constructed. Two sections were formed to begin with in each group; then, in order to secure the steadying effect of larger numbers, the averages of corresponding sections in the two groups were united. In this way we have under each form of memory an *increasing* and a *decreasing* section whose averages are given for each one of the 10 succeeding stages. Here, as in some former cases, the division into two groups has no essential connexion with the problems which are being discussed.

---

[1] It is of course clear that the statistical conclusions which have been brought forward would have been still more reliable with a larger basis of experiments.

TABLE X. *The Successive Stages in Visual and Kinaesthetic Memory.*

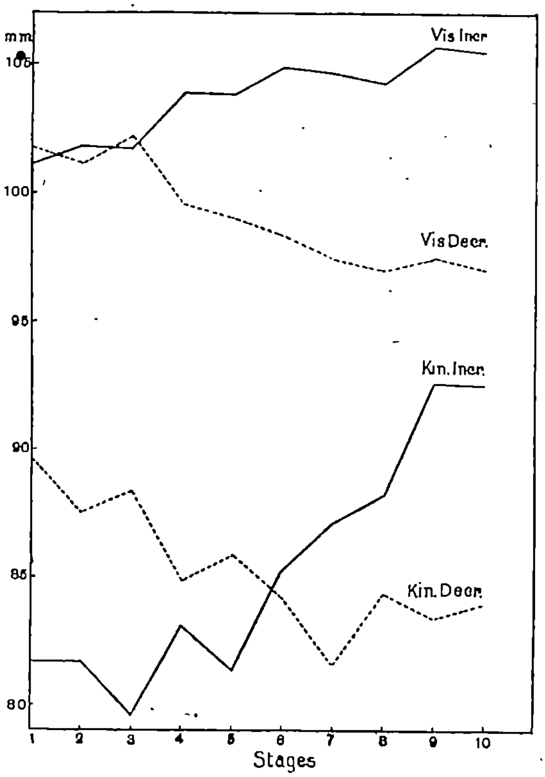
		1	2	3	4	5	6	7	8	9	10	Number[1] in section
Visual	Increasing	101·1	101·8	101·7	103·9	108·8	104·9	104·7	104·8	105·7	105·5	33
	Decreasing	101·8	101·2	102·2	99·6	99·1	98·5	97·5	97·1	97·6	97·1	17
Kin-aes-thetic	Increasing	81·7	81·7	79·6	88·1	81·6	85·8	87·1	88·2	92·6	92·5	28
	Decreasing	89·6	87·5	88·4	84·9	85·9	84·2	81·6	84·4	83·5	84·0	21

In the curves which are given below each increase or decrease of a tenth of a millimetre in the numerical values of Table IX is represented by a rise or fall of half a millimetre in the curve. The starting points of the lines have been chosen in accordance with the absolute magnitudes of the reproduced lines at the first stage; the continuous lines refer to *increase*, whether visual or kinaesthetic, while the dotted lines represent *decrease*. No stress is meant to be laid on any isolated value: on the other hand, it is to be noted that the averages in the section which includes the smallest number of subjects, are based on 17 independent values.

The first values in the decreasing series, visual and kinaesthetic, are higher than those in the increasing series: the difference, however, is slight in one case, and, were it not that it appears in both groups, would hardly be worthy of notice. The kinaesthetic curve is steeper both in increase and decrease, than the visual, though the difference is more marked in the increasing section: it is also more variable and undulating, a characteristic which should no doubt be brought into connexion with the greater general and individual mean variation of kinaesthetic memory. The number of subjects whose lines increase at the later stages is much greater than the number of those whose lines decrease; this fact, however, is more prominent in the sphere of visual reproduction. The results thus show that there is no one exclusive tendency in the successive reproductions: there are two distinct types, and between these two types lies a group in which the tendencies to increase and decrease are less clearly marked.

---

[1] Owing to the difficulty of measuring all the lines of one subject, the total number of the two sections for kinaesthetic memory is 49 instead of 50.



Successive Stages in Visual and Kinaesthetic Memory.

It is of interest to inquire how far these changes are due to special limited conditions and how far they represent a general psychophysical tendency in the individual. As evidence on this point it may be noted that the percentage of reagents whose series, visual and kinaesthetic, change in the same direction, *i.e.* are both increasing or decreasing, is for the combined groups 59·2. Any weight which this value might have is greatly diminished when we find that there is no trace of such correlation in the data of the standard group. Further evidence may be sought in the answer to the question, whether over-estimation or under-estimation in copying the plain line is correlated with the tendency to increase or decrease the length of the remembered line at successive stages. For the purposes of this inquiry those individuals from each group—16 in number—have been chosen who showed the most distinct changes in the way of increase or decrease in memory, and it has then been determined for these subjects how often there was agreement of over-estimation in copying with increase in reproduction, and of under-estimation with decrease. The average of the values for the two groups gives a percentage of 62·5 for copying and visual memory, and 68·8 for copying and kinaesthetic memory. Here again, however, the evidence for correlation is furnished almost wholly by the supplementary group. Finally it is to be noted that when these 16 individuals are again taken, and it is determined in how many cases there was agreement of over-estimation in the final memory average with increase at successive stages and of under-estimation with decrease, it is found that the percentage of agreements for the combined group is 64·1 for visual memory, and 46·9 for kinaesthetic memory. Once more this evidence for correlation in the data of visual memory is furnished entirely by the supplementary group. It must be said then on the basis of these results that there is no adequate proof of any general correlation of these various processes.

In all the four curves of the figure there is a marked alteration between the third and fourth stages. The best available data make the average time taken in the reproduction of each successive line between 10 and 13 seconds; on this showing the alteration referred to would probably lie between 30 and 40 seconds after the beginning of the series. It is probably not without significance that it is thus found to lie in the region of a period of more extensive variation which showed itself in the experiments of Radoslawow-Hadji-Denkow on visual memory[1]. The other variations in the later portions of the curves do not seem sufficiently harmonious to form a basis for further inferences. The range of individual variation in visual memory is small as compared with that of kinaesthetic memory (Table III):

---

[1] *Phil. Stud.* 1899, xv. S. 358.

this is an easily understood feature. Another feature, the smallness of the individual mean variation in visual memory, as compared with that shown in copying the plain line, has already been dealt with.

Tendencies to Over-estimation and Under-estimation in Reproduction.

It has already been seen in Table I that the line drawn by kinaesthetic memory is about 15 mm. shorter than the standard line. That this is not in any way an accidental feature is shown by the fact that it appears with equal clearness in both groups. And when, instead of employing the average, we enumerate the cases in which the reproduced line falls short of the standard line, we find practically identical results for each of the two groups: for the combined group the percentage of such cases is 91. Finally it is to be noted that the under-estimation is not something arrived at in the series of successive stages: it is there from the start. The conditions of the experiments being somewhat different, we cannot make a close comparison between the present results and those of other investigators regarding arm movements, but the resemblance is sufficient to show that we have here a feature altogether different from that ascertained in former researches. Jastrow[1], for example, found that whether a line of 100 mm. in length was first presented by vision, or by arm movement, the second reproducing movement, which was meant to be equal in extent to the first line, was invariably made too long. In Delabarre's experiments[2] the same result appears. In the investigation of Fullerton and Cattell[3] it was found that an arm movement of 100 mm. appeared equal to a second reproducing movement which was on the average 111·8 mm. in length.

The most satisfactory explanation of this divergence lies in utilising a principle formulated by Loeb[4]. In discussing the reasons why some reagents, when they attempt to make a second movement equal to one previously executed by the same arm, make it cover an objectively greater space, while others make the second movement too small, Loeb gives the explanation, that the difference is due to a difference in the distribution of attention. The antagonists of the arm are in a state of greater tension when a more precise movement has to be effected,

---

[1] *Mind*, 1886, O.S. xi. pp. 544, 547.

[2] *Ueber Bewegungsempfindungen*, Inaug. Diss. 1891, SS. 86, 110.

[3] *On the Perception of Small Differences*, p. 48, 1892.

[4] *Pflüger's Archiv*, 1887, xli. S. 126.

i.e. when the attention is specially directed to the movement; in consequence of this the movement tends to be less rapid, and thus less extensive. Some reagents direct their attention mainly to the first, others to the second, reproducing movement; in the former case the first movement will tend to be slow, and therefore less extensive; in the second case the reproducing movement will have these characteristics. Returning to the observations in the present investigation we have to recognise that the task set to the subjects in these experiments, of giving their attention to the movement of the hand while the pattern line was being followed three times, was one which could be carried out only to a very limited extent, owing to the predominance of the visual picture. If this be so, it follows that the succeeding movements of reproduction would receive a far larger share of attention than the original movements and that thus the objective space covered by the latter movements would be less extensive.

In Loeb's statement the emphasis is laid on the duration of movement as the determining factor in the spatial estimation. I should prefer, in explanation of the present results, to lay stress on another factor, viz. the increase in the intensity of the kinaesthetic sensations which must inevitably occur when a movement is executed by muscles which, owing to heightened attention, are in an increased state of tension. Where such an increase in the sum of kinaesthetic sensations occurs it will tend to be interpreted as an increase in the magnitude of the space objectively covered, and therefore the length of the line drawn under these circumstances will tend to be shorter[1].

It is quite possible that different duration of the original and the reproducing movements is an additional factor contributing to the diminution of the reproducing movement. There are not available any direct measurements of the time taken by the subjects in the various movements. But if we make the assumption, which other observations render a probable one, that the total time taken by the subjects in carrying out the determinations is in general a fair index of the rate with which the lines were drawn, we can utilise the data contributed by the students in regard to the time taken in the various operations. The most trustworthy data are those regarding the time required for carrying out the second set of determinations with the plain line and the illusions. Taking these values we find that the average time required was 4 min. 37 sec. in the standard, and

---

[1] Cf. Delabarre, *loc. cit.* S. 82.

4 min. 32 sec. in the supplementary group. If we select those individuals who do their work most quickly (7 in the standard, 8 in the supplementary group) we find that the average lengths of the kinaesthetic reproduction in the two groups respectively are 87·9 mm. and 82·3 mm. The somewhat less trustworthy data for the time taken up in carrying out the kinaesthetic determinations show the same ratio. In other words, those who work quickly tend to draw a longer line than the average in the standard, and a shorter line in the supplementary group. With this evidence in view it may be doubted whether varying rate of work and speed of movement play an important part in these results.

It might further be suggested that a partial explanation of the diminution of the reproducing movement is to be found in the fact that we have here in part a translation of visual into kinaesthetic space. It has, however, already been seen that when this factor is independently studied, as in the work of Jastrow and Delabarre, the space covered by the reproducing movement is made too large. We have good reason then for considering that the values for kinaesthetic memory afford a real indication of the nature of the kinaesthetic processes. The original impression has been obscured by the predominant visual impression; we are not, however, dealing with an essential alteration of reproductive activity when we consider the shortening of the reproducing movement, but only with a reduction of the scale in which it is expressed.

In visual memory the average length of the reproduced line shows a slight tendency toward lengthening. That this tendency is slight is proved not merely by the relatively small constant error, but also by the fact that for the combined groups the percentage of those whose lines are longer than 100 mm. is 51. The lengthening, in fact, seems to be due mainly to the circumstance, which appears in both groups, that when the error is one of drawing too long a line it tends to be a relatively large one[1]. Here again we have evidence of two distinct types, separated by a group of subjects in whom the distinction is much less marked.

These results do not give any support to the idea that we may expect to find in the memory process a general change in the direction either of increase, or of decrease, of the memory image. The fact that the average line in visual memory is somewhat too long is of practically no importance in this connexion. Were there really operative in these results a general tendency towards a progressive alteration in the character of the reproductive process, it surely would follow that, where an individual manifested a tendency to over- or under-estimation in the general average for visual or kinaesthetic memory, this tendency would show itself also with some degree of constancy in a progressive change during the course of the ten determinations. There would be, in other words, a close correlation between the tendencies toward over-estimation and increase on the one hand, and under-estimation and decrease on the other. But, as has already been proved, there is no satisfactory evidence of the correlation of these factors. What does seem to be of great importance is the correlation already shown to hold between the character of normal estimation and the character of memory. This shows that the processes occurring in reproduction are quite incorrectly understood when thought of in relation exclusively to memory; another, and, as we have seen, an exceedingly important factor, is the inherent tendency of the individual to draw a long or a short line, whether he is copying or remembering. It is in some ways a true and fruitful mode of conception to look on the activity in reproduction and reconstruction of experience as a specialised form of purposive reaction to stimulus, whether that stimulus appears as direct impression or as idea. From such a point of view one can easily understand how the reaction or response is determined in part by the particular magnitude and character of the stimulus, but in part also by the fundamental character, or pre-existing bias, of the central psychophysical processes. If this fundamental character, for example, is in the direction of exaggeration, then the line drawn in reproduction will tend to be relatively long, whether the special form of reproduction be visual or kinaesthetic. It is evident also that the same mode of consideration can be applied to the phenomena of increase or decrease at successive stages; the central reacting mechanism varies in the character of its discharges on successive stimulation. Such a view is incomplete and requires supplementation, but it helps us to avoid the artificiality involved in attempting to explain reproduction solely in terms of memory images and ideas.

---

[1] It should be mentioned that in the small group whose results appear in Table IV there are 7 subjects whose lines drawn in copying are over 100 mm., and 8 subjects whose lines drawn by memory are over 100 mm.

The Operation of Contrast and other Factors at Successive Stages.

In view of the circumstance that the twelve determinations of the plain line *A* were carried out, not continuously, but in sets of three, it seemed best to calculate the average value for each set of three lines instead of for each line. This has been done for the standard group, with the following result. If we take *m* as the mean value in the first set of three lines, then the values in the three following sets in order are, $m - 1{\cdot}0$ mm.; $m - 1{\cdot}5$ mm.; *m*. This shows in general that there has been no constant tendency towards increase or decrease in estimation. It is also interesting in another aspect. It will be remembered that the order in which the lines were drawn was, both before and after the interval, *A*, *B*, *C*; *A*, *C*, *B*. The question then suggests itself: does the drawing of the *A* line at the beginning of the first and third sets, where it is free from all possible effects of contrast, differ materially from the drawing in the second and fourth sets when it follows the presentation and copying of the diminishing form of the illusion? It will be noted that while *A* in the second set is shorter by 1·0 mm. than in the first set, it is in the fourth set larger by 1·5 mm. than it was in the third set. In other words, there is no evidence that there is any sensible effect of contrast in these experiments.

In the investigation of Binet, already referred to, the agency of successive contrast is invoked to explain certain peculiarities in his results. In his experiments—to take one example of the general tendency—a series of lines, varying in length from 9 to 15 cm., which were modified by the diminishing illusion, were presented in succession to each child: out of this series a line had to be selected as being equal to one which was modified by the magnifying illusion. When the series began with the shorter line, 9 cm., the magnitude of the illusion was found to be 1·85 cm.; while if the series began with the line 15 cm. in length the magnitude was 1·92 cm. This difference is attributed to contrast in this way, that when the lines successively presented increase in length each line is taken through contrast to be longer than it really is, and thus the point of apparent equality is reached sooner than it otherwise might be; the result of this is an increase of the illusion. A similar process operating in the decreasing series tends to lessen the illusion by leading the subject to select a line which is relatively long. Such an explanation is no doubt possible, but it seems unnecessary in view of the circumstance that the results above referred to can be quite well explained by the influence of expectation. This agency, whose effectiveness is well recognised, would cause the individual in each case to choose a line too near the beginning of the series; and this is what the reagents in Binet's experiments have done. It would seem, therefore, that neither in these experiments, nor in the data of the present investigation, does it seem necessary to assume the operation of successive contrast.

Does the effectiveness of the two forms of illusion alter in successive determinations? In order to answer this question, the average magnitude of the *B* and the *C* lines has been determined for the first and the fourth set of experiments in the standard group. It is found then that the average copy of the *B* line is longer in the last set by 2·5 mm. and that of the *C* line is shorter by 1·5 mm. Since we have found no evidence of contrast, and since the *A* line, as already shown, is copied in the same way in the last set as it is in the first, we have good reason to attach some weight to this fact. It would seem then that there was a distinct tendency to increase the magnitude of each form of the illusion. In Judd's experiments[1] regarding the effect of prolonged practice in estimating the Müller-Lyer illusion, its effectiveness was found to diminish greatly after a time. In the present case the increasing familiarity and practice due to successive estimations have had no influence in diminishing the effectiveness of the illusions; if there is any change it is in the direction of heightening their effect. It seems reasonable to connect this increased effectiveness of the illusions in the last determinations with the circumstance that in the first determinations which followed immediately on the tracing of the pattern lines the subjects would inevitably be more clearly conscious of the main line as the central feature in each form of the illusion.

General Conclusions.

1. The conclusions of former investigations regarding the character, relative magnitude, and prevalence of the two forms of the Müller-Lyer illusion are confirmed.

2. When the estimation of the plain line is compared with that of the two forms of illusion it is found, (*a*) that there is no definite alteration in the homogeneity of the standard group (general mean variation) through the action of the illusion, but that in the supple-

---

[1] *Psychol. Rev.* 1902, IX. p. 27.

mentary group, which has a higher initial range of variation, there is an alteration due to this action; (*b*) that individual variability (individual mean variation) is heightened by the action of the illusions in both groups.

3. There are certain inherent, or preexisting, psychophysical tendencies towards exaggeration or over-estimation, and diminution, or under-estimation, which are found operative whether the individual is copying a plain line, or is reproducing it by visual and kinaesthetic processes, or is measuring a standard line of 100 mm.

4. There appears to be a certain degree of direct positive correlation between the magnitude of the lines drawn in copying the plain line and the magnitude of the diminishing form of illusion, and of inverse correlation between the length of the plain line and the magnifying form of illusion. These phenomena are formulated in the principle that each form of illusion tends to reach its maximum effectiveness where the preexisting tendency is in the opposite direction.

5. There is no adequate evidence of direct correlation of the two forms of illusion: there is some evidence of inverse correlation, which may be taken as being due to the operation of the principle already stated.

6. Correlation of the two forms of illusion with visual and kinaesthetic memory is irregular, but so far as the prevailing tendencies go, they are the same in character as those formulated in par. 4; each form of illusion tends to reach its maximum where the tendency shown in memory is in the opposite direction. This is interpreted as an expression of the relations indicated in par. 3.

7. There is fairly definite proof in the standard group that accuracy in estimation of the plain line is accompanied by lessened subjection to the illusions and by increased accuracy in visual and kinaesthetic memory.

8. Increased accuracy in work is not accompanied by increased steadiness and uniformity of work as measured by the individual mean variation. Variation in accuracy, alteration in the constant error, is interpreted as being due to change of the special sensory factor in perceptive activity: the individual variability is taken broadly as an index of the general factor of attention.

9. The supplementary group, constituted on the basis of less perfect drawing of the figures embodying the Müller-Lyer illusion, is characterised throughout its work by increased variability and decreased coordination.

10. There appear to be two distinct types in reproduction; those whose average reproduction is greater than the mean value for the group and those whose reproduction is less, and there is a high degree of direct correlation of these types in the two forms of memory. There appears, however, to be no clear evidence of any relation between the tendencies to over-estimation and under-estimation and those towards increase and decrease at successive stages.

11. The first lines drawn by those whose reproductions increase at the later stages tend to be shorter than the lines drawn by those whose later reproductions decrease. There appears to be a marked alteration in the reproductive process between the third and fourth stages. The range of general and individual variation is greater in visual and kinaesthetic memory than it is in copying the plain line.

12. There is no evidence of the operation of contrast in the successive determinations, or of any definite alteration in the estimation of the plain line. There is evidence that the magnitude of each form of illusion is greater in the last set of determinations than it is in the first.

IMMEDIATE MEMORY IN SCHOOL CHILDREN.

No. II. AUDITORY.

By W. H. WINCH.

I. *The problem stated.* II. *The method of experiment and its validity.* III. *Experiments made in* 1905—1906. IV. *Suggested conclusions.* V. *Table of comparison between memory and general proficiency.*

I. The following series of experiments were made in continuation of the work on visual memory described in the *British Journal of Psychology*, Vol. I. p. 127. The questions now raised are identical with those then considered, viz.

1. Whether pure memory, that is, memory of percepts associated only in time and space, is improvable by practice.

2. Whether such memory tends to improve as the children rise in age and standard [of mental proficiency] in school studies.

3. Whether such memory has any relation to general intellectual proficiency as measured by the position of children within their classes.

II. *The method employed.* Sets of consonants, 12 for each exercise, were called out by me in a loud, clear voice, each consonant being carefully separated from the next, with a slightly more marked separation between the 4th and 5th and between the 8th and 9th. After some preliminary practice I succeeded in dictating the consonants so that two repetitions occupied exactly 25 seconds. The children then reproduced in writing as much as they could remember, 1 min. 35 secs. being allowed before the next exercise was called out.

Is the method valid as a test of memory free from all associations other than those of temporal antiquity? To study this point, the children were asked at the conclusion of the 5th Set of tests if they had thought of any words or sentences when hearing the letters. Some said they had and were questioned separately with the following results: in Standard III, D. K. thought of 'gum' and 'buy,' the letters being *g b d y*, *x p l m*. The association misled her and she wrote *g b y m*. In Standard V, E. K. was led, in one paper, to think of 'Sir' and 'Ted' by the letters *s r t d*; these consonants were correctly remembered, and in another of 'vap' and 'bin' the consonants being: *b q v p*, *b n* etc.: these consonants were also correctly remembered. In Standard VI there were two girls who had thought of words. W. B. thought of 'kilt,' the consonants being *k t l p*. M. W. thought of C. I. V., the consonants being *c v*, and 'tax,' the consonants being *t q*; the association was misleading in this case, the consonants being *t q*, whilst the child wrote *t x*. She also thought of 'sin' and 'red,' the consonants being *s n* and *r d*. In Standard VII, C. W. thought of 'pen,' the consonants being *p n*, of 'lick,' the consonants being *l q*, and of 'kettle,' the letters being *k t l*. A. S. thought of 'gas,' the consonants being *b s*, this misled her and she wrote *g s*. She also thought of 'rum,' the letters being *r m*, and of 'yam,' the letters being *y m*.

Summarising these facts, we find that only 6 girls out of 36 made any associations of this kind, and even then in only one or two of the ten exercises given on that date.

Is the method valid as a test of the improvability of 'pure' memory or does it measure an improvement in facility apart from, or in addition to, retentiveness?

A friendly critic has suggested to me that by such a method we may only be testing an improvement in knowing how to set about the work, in which, as he says, we should expect those who are mentally more mature to make more rapid headway than those of inferior powers.

In the first place, I tried to overcome the influence of any such factor by preliminary exercises, with no time limit, in which the children were carefully instructed and practised in what they had to do. Nor would the contention survive after a close scrutiny of the individual exercises, which only lack of space prevents me from giving here. It is found that the individual children do as well at the beginning of each day's work as later on in the series given on the same day. And this is true even for the first day's work.

III. The exercises on which the conclusions suggested in this paper are based were given to selected girls in a Council School situated in a good neighbourhood of a London suburb, commencing on December 6th, 1905. Ten auditory memory exercises were given on each occasion, the next set being given one week later, on December 13th, the next on December the 20th, and then after an interval, partly necessitated by the Christmas holidays, a fourth series on January 17th and the final series on January the 31st. All the exercises were given on the same day of the week at 10.20 a.m.

The consonant tests, of which the following is a specimen,

y p j f, c t b s, h r q w,

were marked as in the visual exercises already referred to, except that no marks were allowed for vertical displacement.

Children were selected from six school classes ranging from II to VII and were chosen by the Head Teacher so as to be as far as possible representative of the average age and ability in each standard, though an exact determination of class position, subsequently made, showed that each class was represented by a group of children who were, on the whole, above the median position. This position in class was finally fixed by the ordinary terminal examination held by the Head Teacher in all subjects of school instruction at Christmas 1905.

The results are given in Table I:

IV. 1. There seems to be a marked and almost invariable improvement, slightly interrupted in some cases by the interval of holiday.

2. School proficiency, as measured by age and standard and position in school, appears to be generally accompanied by good memory of the kind we are here testing, though not invariably so.

3. That when the comparison is narrowed to children of the same grade or standard, it is still found that memory is positively related to position in school, though, as we should expect, with less exactitude than when a wider range of age and standard of school proficiency is taken for comparison.

As the later point may not be so easily seen from the cases given, the range in each class being hardly wide enough, I made a special experiment directed solely to that issue.

Two sections of children in Standard IV were taken, composed

(*a*) Of six children at the top of the class excluding those who had previously done any tests and, of course, any who were absent on the day in question.

TABLE I.

Table of 5 Sets of Tests		Age on 31 7 05		Average of 10 Consecutive Tests	Average of 10 Consecutive Tests	Average of 10 Consecutive Tests	Average of 10 Consecutive Tests	Average of 10 Consecutive Tests	Total	Position in class	Percentage Position
Name	Standard	Years	Months	Dec. 6	Dec. 13	Dec. 20	Jan. 17	Jan. 31	Average	Xmas '05	in Class
D. C.	II	7	2	12·0	12·0	14·8	9·7	13·3	*12·4*	1 out of 67	1·5
W. M. ...	,,	7	7	9·4	8·7	8·7	10·3	11·7	*9·8*	8 ,, 67	11·9
N. G.	,,	7	5	10·5	10·0	9·0	9·6	9·7	*9·8*	11 ,, 67	16·4
M. F.	,,	7	0	2·5	5·2	8·9	8·5	*a*	*6·3*	18 ,, 67	26·9
C. B.	,,	7	2	6·8	7·9	10·8	9·0	14·1	*9·7*	27 ,, 67	40·3
R. W.	,,	7	0	3·0	6·3	9·0	10·0	10·8	*7·8*	28 ,, 67	41·8
		7	*3*	*7·4*	*8·3*	*10·2*	*9·5*	*11·9*	*9·3*	15·5 ,, 67	23·1
D. K.	III	7	6	4·3	10·2	14·9	15·2	11·5	*11·2*	1 out of 65	1·5
M. S.	,,	8	7	9·9	10·9	11·8	13·4	15·0	*12.2*	1 ,, 65	1·5
M. F.	,,	8	1	9·2	11·1	11·7	10·8	13·4	*11·2*	11 ,, 65	16·9
W. M. ...	,,	8	8	9·4	10·0	8·0	9·7	10·2	*9·5*	11 ,, 65	16·9
E. J.	,,	8	7	7·1	8·1	13·3	12·0	13·0	*10·7*	11 ,, 65	16·9
K. W. ...	,,	8	0	6·7	9·4	11·0	8·6	8·8	*8·9*	11 ,, 65	16·9
		8	*3*	*7·8*	*9·9*	*11·8*	*11·6*	*12·0*	*10·6*	7·7 ,, 65	11·7
S. K.	IV	9	10	12·7	11·2	16·2	14·2	19·3	*14·7*	2 out of 50	4
D. F.	,,	9	5	10·2	13·0	19·0	16·2	19·7	*15·6*	2 ,, 50	4
B. F.	,,	9	11	8·2	11·1	13·0	12·4	13·6	*11·7*	4 ,, 50	8
A. H.	,,	9	10	10·6	11·2	16·0	13·0	13·4	*12·8*	11 ,, 50	22
D. T.	,,	9	7	11·7	18·3	*a*	14·4	18·9	*15·8*	14 ,, 50	28
D. T.	,,	9	5	8·1	12·3	11·0	14·9	11·1	*11·4*	24 ,, 50	48
		9	*8*	*10·2*	*12·8*	*15·0*	*14·1*	*16·0*	*13·6*	9·5 ,, 50	19·0
G. K.	V	10	9	15·4	19·1	17·0	16·7	20·0	*17·6*	1 out of 50	2
A. H.	,,	10	7	13·9	12·7	15·1	15·0	16·6	*14·6*	2 ,, 50	4
G. W. ...	,,	11	0	11·3	12·9	13·9	17·4	16·7	*14·4*	6 ,, 50	12
W. B. ...	,,	10	5	12·7	*a*	15·0	15·2	15·8	*14·7*	15 ,, 50	30
M. M. ...	,,	10	10	12·0	*a*	*a*	11·6	13·3	*12·3*	15 ,, 50	30
R. T.	,,	10	11	7·8	13·7	13·8	15·4	15·2	*13·2*	19 ,, 50	38
		10	*9*	*12·2*	*14·6*	*15·0*	*15·2*	*16·2*	*14·5*	9·7 ,, 50	19·3
M. W. ...	VI	11	7	9·8	19·5	22·1	21·0	22·5	*19·0*	1 out of 53	1·9
M. L.	,,	11	5	11·2	13·4	13·7	15·2	15·9	*14·0*	2 ,, 53	3·8
E. W. ...	,,	11	3	5·1	6·5	14·2	11·5	17·2	*10·9*	6 ,, 53	11·3
W. B. ...	,,	11	11	11·4	18·4	17·9	18·9	20·4	*17·4*	9 ,, 53	17·0
L. B.	,,	11	0	8·9	13·3	13·4	14·3	*a*	*12·5*	17 ,, 53	32·1
K. E.	,,	10	5	8·5	10·2	15·2	15·8	18·0	*13·5*	20 ,, 53	37·7
		11	*3*	*9·2*	*13·5*	*16·0*	*16·1*	*18·8*	*14·6*	9·2 ,, 53	17·3
M. K. ...	VII	12	5	13·5	20·3	23·3	25·9	24·9	*21·6*	1 out of 27	3·7
C. W. ...	,,	13	2	15·4	23·9	25·4	28·0	30·7	*24·7*	1 ,, 27	3·7
G. W. ...	,,	13	2	17·5	28·8	26·9	*a*.	22·1	*23·8*	5 ,, 27	18·5
A. S.	,,	13	0	15·2	17·6	17·5	16·0	19·2	*17·1*	8 ,, 27	29·6
E. Y.	,,	13	2	11·8	17·2	18·3	17·3	17·1	*16·3*	15 ,, 27	55·6
S. W.	,,	13	2	15·0	17·6	17·5	22·7	23·1	*19·2*	24 ,, 27	88·9
		13	*0*	*14·7*	*20·9*	*21·5*	*21·9*	*22·8*	*20·4*	9·0 ,, 27	33·3

(*b*) Of six children at the bottom of the class excluding, as before, any previously tested and absent children.

The results are given in Table II.

SPECIAL GIRLS. TABLE OF TWO SETS OF TESTS. II.

Rank in class order	Names of 6 best	Rank in memory order	Standard	Ages 31 7 05		Average of 10 Consecutive Tests	Average of 10 Consecutive Tests	Total Average	Position in Class Xmas 1905	Percentage Position
				Years	Mths	Jan. 17	Jan. 31			
1	W. B.	9	IV	8	9	10·9	11·1	*11·0*	1 out of 50	2
2	L. H.	1	,,	11	8	14·2	20·0	*17·1*	3 ,, 50	6
3	D. C.	5	,,	11	6	11·6	14·0	*12·8*	3 ,, 50	6
4	A. E.	2	,,	12	4	14·9	17·8	*16·3*	6 ,, 50	12
5	N. J.	3	,,	11	7	14·2	16·4	*15·3*	11 ,, 50	22
6	G. W.	4	,,	9	6	14·2	14·8	*14·5*	17 ,, 50	34
	Names of 6 worst			*10*	*10*	*13·3*	*15·7*	*14·5*	6·8 out of 50	13·6
7	N. P.	7	IV	11	3	11·1	12·8	*11·9*	41 out of 50	82
8	E. T.	12	,,	12	6	7·1	11·0	*9·0*	41 ,, 50	82
9	H. K.	11	,,	13	2	7·2	11·0	*9·1*	47 ,, 50	94
10	E. G.	9	,,	13	8	8·2	13·6	*11·0*	48 ,, 50	96
11	G. T.	8	,,	11	10	9·9	13·0	*11·4*	49 ,, 50	98
12	E. M.	6	,,	11	2	11·7	13·1	*12·4*	50 ,, 50	100
				12	*2*	*9·2*	*12·4*	*10·8*	46 ,, 50	92·0

There is a rather remarkable case in No. 1, St. IV in which a low rote memory accompanies high ability, but on the whole the relation is a positive one. It is less marked, however, than the correspondence between class position and memory in the girls of some of the previously selected groups. I incline to attribute this to the relative fewness of the exercises.

V. It has been suggested to me that it would be well to arrange the results in a form which would render easy an application of numerical methods of correlation, even if it might be maintained that such formulae are not exactly applicable to psychical variations. I accept the suggestion and append the following Table. The rank in class order and memory order has already been inserted in Table II.

TABLE III.

Standard	Name	School order in general proficiency	School order in memory	Class order in general proficiency	Class order in memory
VII	M. K.	1	3	1	3
,,	C. W.	1	1	1	1
,,	G. W.	3	2	3	2
,,	A. S.	4	8	4	5
,,	E. Y.	5	9	5	6
,,	S. W.	6	4	6	4
VI	M. W.	7	5	1	1
,,	M. L.	8	16	2	3
,,	E. W.	9	28	3	6
,,	W. B.	10	7	4	2
,,	L. B.	11	20	5	5
,,	K. E.	12	17	6	4
V	G. K.	13	6	1	1
,,	A. H.	14	14	2	3
,,	G. W.	15	15	3	4
,,	W. B.	16	12	4	2
,,	M. M.	16	22	5	6
,,	R. T.	18	18	6	5
IV	S. K.	19	12	1	3
,,	D. F.	19	11	1	2
,,	B. F.	21	24	3	5
,,	A. H.	22	19	4	4
,,	D. T.	23	10	5	1
,,	D. T.	24	25	6	6
III	D. K.	25	26	1	2
,,	M. S.	25	23	1	1
,,	M. F.	27	26	3	2
,,	W. M.	27	33	4	5
,,	E. J.	27	29	5	4
,,	K. W.	27	34	6	6
II	D. C.	31	21	1	1
,,	W. M.	32	30	2	2
,,	N. G.	33	30	3	2
,,	M. F.	34	36	4	6
,,	C. B.	35	32	5	4
,,	R. W.	36	35	6	5

THE WHEATSTONE-HIPP CHRONOSCOPE. ITS ADJUSTMENTS, ACCURACY, AND CONTROL.

By BEATRICE EDGELL and W. LEGGE SYMES.

(*From the Physiological Laboratory of the University of London.*)

Part I. Introductory and Historical.

Part II. The Adjustments of the Chronoscope.

(*A*) *Graphic study of their effect on the Armature Movements.*

(*B*) *Observations with Wundt's hammer.*

Part III. The accuracy and control of the Chronoscope. General Conclusions.

PART I. INTRODUCTORY AND HISTORICAL.

THE 'Hipp' Chronoscope is such an important piece of apparatus for psychological purposes that no apology is needed for adding another paper to the list of those which deal with its adjustment and control, if an independent investigation of the instrument *ab initio* has resulted in even one small item of new information.

The purpose of the present investigation has been to determine the conditions and degree of accuracy in chronoscope readings.

We have endeavoured to arrive at these determinations by:

(*a*) Graphic study of the movements of the armature of the electromagnets in the chronoscope.

(*b*) Control of chronoscope by chronograph readings.

The following brief historical notes will sketch the origin and early use of the instrument and will also serve to indicate the relation of our own work to that of previous investigators.

The chronoscope is of English parentage. In 1840 Wheatstone (1) [1], on the principle of his electromagnetic telegraph, constructed a clock in which the hands were set in motion and checked by the action of an electromagnet on its armature. The divisions on the dial of the clock recorded $\frac{1}{60}''$ or $\frac{1}{100}''$, according to the adjustment of the weight which drove the clockwork.

---

[1] For Bibliography see end of Paper.

This clock was used in gunnery to determine the velocity of projectiles. The snapping of a thin wire, stretched across the mouth of a gun, served to break an electric circuit (to start hands of clock), while the impact of the shot on the target remade it (stopped hands of clock).

Wheatstone was conscious of the error which might arise through delay in release of armature, *i.e.* error of remanent magnetism. To avoid it, he arranged that the current whose circuit was broken by the snapping of the wire should be weak, whereas the current whose circuit was closed by impact on the target should be strong. He thus secured rapid response to make and break, one of the fundamental conditions for chronoscope accuracy.

In 1842 Mathias Hipp, who at that time was a watchmaker and mechanic in Reutlingen (Wurtemburg), visited Prof. Eisenlohr (2), the physicist, at Karlsruhe, and was there shown a model of Wheatstone's instrument. This gave Hipp the idea of his Chronoscope, an account of which was published in 1848 by Oelschläger (3), Oberreallehrer in the Polytechnic at Reutlingen. Hipp improved upon the original by separating the mechanism of the hands from the rest of the clockwork, thus eliminating the error due to acceleration of clockwork.

In this model Hipp used as regulator a vibrating spring, making 500 complete vibrations a second.

In a later model described by Oelschläger (3)[1] (1849), the vibrating spring makes 1000 complete vibrations a second, and the clock dials record $\frac{1}{1000}''$.

It was not until much later that Hipp introduced the second pair of magnets, giving what is known as the 'new model[2].'

With his chronoscope Hipp used as a control, an apparatus which can likewise claim an English ancestor, since it was modelled on Atwood's machine. In this instrument the release of a small metal ball breaks a circuit which is remade by the fall of the ball upon the footboard of the apparatus.

---

[1] Among the various excellences of Hipp's chronoscope, O. remarks that it only costs 66 fl., whereas the English one costs 144 fl.

[2] For full description of chronoscope and its working parts, see Wundt, *Grundzüge der physiologischen Psychologie*, 4te Aufl. Bd III. p. 387, or less fully, Titchener, *Experimental Psychology*, Vol. II. Pt ii. p. 326.

Hipp's first chronoscope was supplied to Prof. Reusch of Stuttgart in 1843[1], but the first account which we have been able to find of its use in scientific investigation is that given by the astronomer Dr A. Hirsch (4) of Neuchâtel. Having used it successfully in his astronomical observations, Hirsch applied it to physiological purposes in 1861. He had the advantage of personal acquaintance with Hipp[2], and seems to have known more about the correct adjustment of current intensity than many of the later workers. He gives tables showing that the mean error of chronoscopic readings is under 2σ.

With the first edition of Wundt's *Grundzüge der physiologischen Psychologie* in 1874 begins the history of the chronoscope in psychological literature. Constancy of current is the condition emphasized by Wundt in the early editions of the *Grundzüge*. He alludes to the fact that Hipp supplied a galvanometer with his instrument, and it is as a further check on varying current that Wundt recommends the use of the control apparatus.

J. Merkel (5), 1885, gives an empirical rule as to the intensity of current which should be used: it should be just strong enough to arrest the hands.

Berger (6), 1886, works out the influence of intensity on current reading. He shows that the stronger the current which causes arrest of hands, the shorter the time recorded by the chronoscope. Incidentally he also shows that the difference between control time and chronoscope time, *i.e.* the error of his chronoscope, is the same with long control intervals as with short.

J. Cattell (7), 1886, states the fundamental condition for elimination of the electro-magnetic error, an error which he points out may amount to as much as $\frac{1}{10}''$. This condition is adjustment of spring tension in the chronoscope and current intensity, in such wise that the times between make of current and attraction of armature and between break of current and release of armature shall be equal. This is tantamount to recognizing that equal latencies is the condition of accuracy, but Cattell does not give a detailed analysis of the factors which may affect latency. His further investigations are directed to the perfecting of a control apparatus—the aim of most of the subsequent writers on the chronoscope.

---

[1] We are indebted to M. v. Peyer of the firm of Peyer and Favarger, successor to M. Hipp in Neuchâtel, for this and other points concerning the evolution of the present chronoscope.

[2] Hipp was at this time established at Neuchâtel, as Director of the Telegraphic Factory, and himself acted as 'subject' in Hirsch's experiments with the chronoscope.

L. Lange (8), 1888, in a paper on Reaction-Times gives explicit recognition to the need of determining the "electro-magnetic correction," and regards current intensity as the chief factor affecting this, although he suggests that it may not be independent of the duration of the current. G. E. Müller (9), 1891, bases a criticism of a piece of work by Münsterberg on the last fact, contending that the intensity of current and adjustment of chronoscope which gives a correct reading for a short period, 150 σ, does not necessarily give one for a longer period, owing to the influence of remanent magnetism[1].

Kraepelin (10), 1892, disputes this contention. He gives tables of experiments wherein the current was allowed to run for 5″, and of experiments wherein the current was allowed to run for 1″, showing that there was very little difference in reading. The worth of this result as a refutation of Müller's statement may be estimated by reference to p. 70 of the present article.

In 1893 appeared the most thorough and valuable of all the investigations into the conditions of accurate reading. This was the paper by Külpe and Kirschmann (11), setting forth their experiments with the then new form of control apparatus, viz. the Wundtian Hammer.

By reference to the curve which may be supposed to represent the growth and decrease of magnetism, the authors trace out theoretically the part played by intensity of current, spring tension, and remanent magnetism in the movements of the armature. They note the point that their chronoscope ran too fast—1024 σ for every second, but they do not trace out the effect of this faulty running on readings of intervals of different duration.

Their tables of readings obtained with the old pattern Hipp show that the accuracy of the chronoscope is much greater than had been supposed.

R. Sommer (12) expresses great confidence in the accuracy of the chronoscope when properly adjusted. It is worth noticing that he is satisfied with the Hipp control apparatus, and that he attributes the irregularity of reading to faulty circuit and bad adjustment. He also speaks of correcting error by a "better adjustment of the regulating (escapement) spring." Müller and Schumann (13), 1900, throw further light on the influence of duration. Working with circuit arrangement wherein the chronoscope records the interval during which there is complete breach of current, and using Schumann's control apparatus, they compared short periods 106—120 σ with longer periods of 722—3800 σ. Their results show that adjustments which gave correct readings for the short intervals gave incorrect readings for the longer intervals. A further question is whether this error is due to remanent magnetism or to faulty running of clockwork. From the fact that the increase of error is proportional to the length of interval, Müller and Schumann conclude that the error is due to faulty running, not to remanent magnetism, as the latter error would, after attaining a maximum, remain constant.

---

[1] Münsterberg's reply (*Beitrage zur Exper. Psych.* 1892, Heft 4), that this criticism could not apply to his particular piece of work, is not altogether to the point. His results will be free from error, not because they are unaffected by remanent magnetism, as he seems to imply, but because the error of remanent magnetism will be constant (with his arrangement of circuit) for all periods. Cf. p. 70 of present article.

N. Ach (14), 1905, has published, as a supplement to his book, *Ueber die Willenstätigkeit*, an account of experiments on the chronoscope with a new control apparatus. This appeared during the later progress of our own experiments and anticipates some of our results. By use of graphic registration Ach is able to measure the amount of error in reading which is due to unequal latencies, and to separate this from error in running of clockwork. Further references to his apparatus and his results are given in connexion with our own work.

It is thus seen that previous writers have recognized, as factors influencing chronoscope readings: Intensity of current, Remanent magnetism, Spring tension, and Accuracy of clockwork. The work in the following pages will analyse these respective factors, and show the effect of mechanical inertia, and demonstrate the influence of a further factor, viz. Resistance of circuit; this being considered apart from its effect on intensity of current.

Three chronoscopes (A, B, and C,) have been at our disposal, manufactured respectively by Peyer and Favarger, Zimmermann, and Hipp himself. For two of these we are indebted to the kindness of Drs W. H. R. Rivers and C. S. Myers. Our ultimate criterion of accuracy has been the tuning-fork. It has not been necessary to go behind this, since its error (even if material) will not affect our deductions.

PART II. THE ADJUSTMENTS OF THE CHRONOSCOPE.

A. Graphic records of their effect on the armature movements.

We have analysed these effects by attaching a light lever to the armature, and recording its movements on a drum which automatically made and broke the chronoscope current.

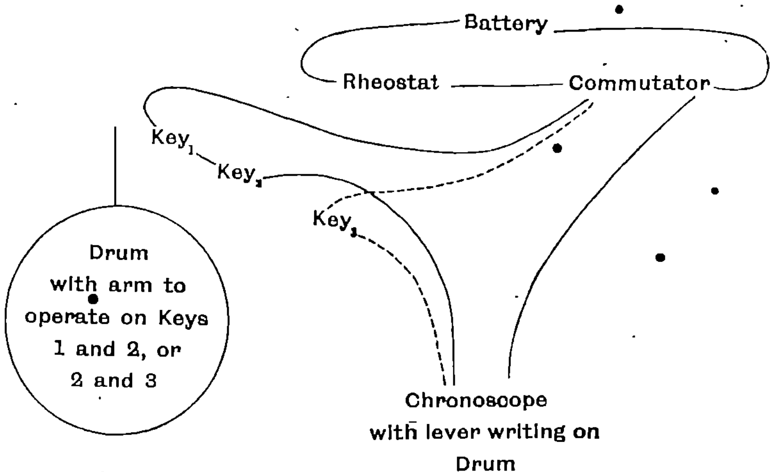

Battery
Rheostat
Commutator
Key$_1$
Key$_2$
Key$_3$
Drum with arm to operate on Keys 1 and 2, or 2 and 3
Chronoscope with lever writing on Drum

FIG. I.

Showing the arrangement of the work table. When the hands of the chronoscope run during current flow, the arm rotating with the drum closes key$_1$ and opens key$_2$. Key$_3$ and its connexions (dotted lines) are not then in use. With hands running during interruption of current, key$_1$ is removed from the drum and closed by hand, whilst the drum opens key$_2$ and closes key$_3$. Key$_1$ (or key$_3$) was the upper key from Wundt's hammer. Key$_2$ was a simple break key.

The lever was cemented to the upper surface of the armature, between the coils of the upper magnet, and projected 14 cm. from its edge. It weighed 1·3 gm. and its centre of mass was within 4 cm. from the edge of the armature; its moment of inertia was therefore a small fraction of that of the total moving mass (armature, counterpoise, &c.). Its effect on the chronoscopic readings was to increase (or diminish) them by about 1 σ, without sensibly adding to their mean variation.

Fig. II. serves to explain the records, all of which read from left to right. The first of the two tracings was taken with the lower magnet in use (hands running during current flow); the second with the upper magnet (hands running during interruption of current). In the latter, the current was made, in the first instance by hand (not shown in the tracing), then broken and re-made by the drum. The hands were running whilst the tracing point was on or below the central horizontal line. This line was obtained by rotating the drum (with current broken, and clockwork running), after the hands had been made to 'crawl,' by manipulation of the spiral springs. It shows therefore the moment of release of the hands. Similar attempts to mark the point at which arrest commenced, produced a line confluent with the above. The starting and stopping points were therefore taken to lie on the same horizontal line. When not otherwise stated, only one of the spiral springs (viz. that which worked against the current), was in use; the other stood at its 'zero.'

In his measurements of the latencies of the chronoscope, Ach recorded the make and break of the current on a drum by an electro-magnetic signal in a circuit which was closed "simultaneously" with the chronoscope circuit. He marked the start and stop of the hands, on the same drum, by an ingenious key (in the chronoscope itself), which worked in a third circuit also containing an electro-magnetic signal. His tracings are not reproduced in his paper.

Our numerical results, obtained by a simpler, and apparently more accurate method, agree in the main with his, and confirm his statement that both the starting and stopping latencies are influenced by changing either the tension of the opposing spring, or the intensity of the current.

The nett effect of such changes may be summarised as follows. Whether the current starts the hands or stops them, their latency at make of current is shortened, and that at break of current is lengthened, either by diminishing the opposing spring tension, or by increasing the intensity of the current, and *vice versâ*. When the

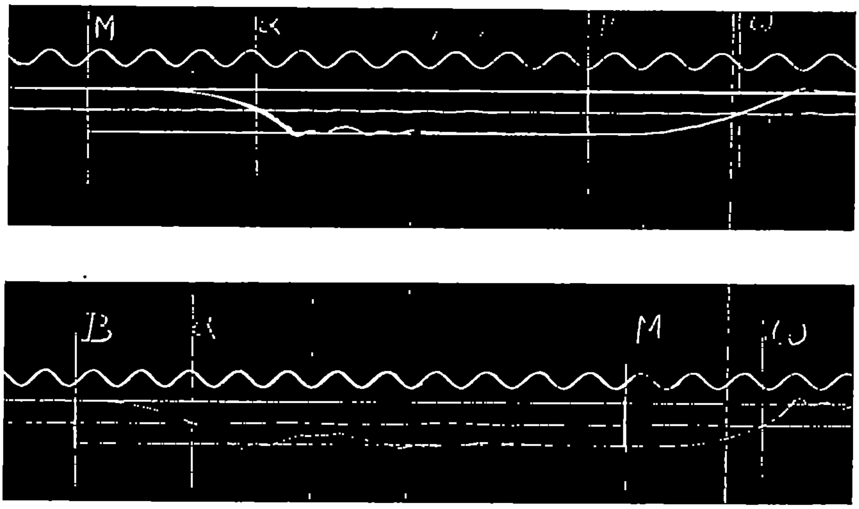


Fig. II.

Graphic records of armature movements. The upper abscissa is that from which the lever starts at make (or break) of current; the lower, that to which it ultimately attains during current flow (or interruption of current), and from which it starts when the current is broken (or re-made). *M* shows make, *B* break, of current. α and ω indicate respectively start and arrest of hands. Tuning-fork 200 per sec. In the upper tracing hands run during flow; in the lower tracing, during interruption of current. The middle horizontal line is explained in the text.

hands run during current flow, their latency at make is the latency of their start, and their latency at break is the latency of their arrest. Hence, diminishing spring tension (or increasing current) will start them sooner, stop them later, and so prolong the reading. When, however,

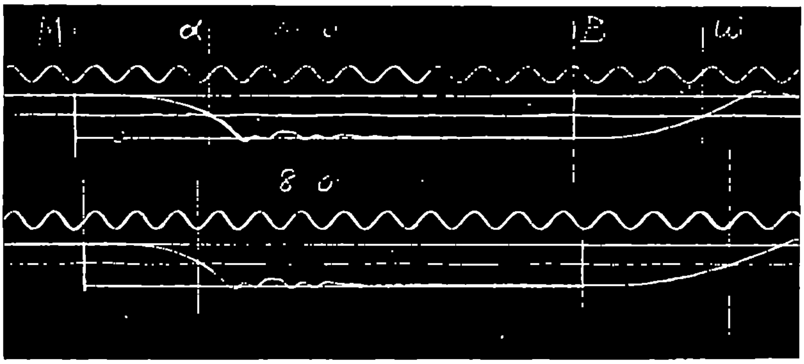


FIG. III.

Tracings of armature movements, to show effect of varying spring tension. Hands running during current flow. Upper spring at 10 in upper tracing, at 8 in lower. Lower spring at zero in both. Letters as in Fig. II. Tuning-fork, 200 per sec.

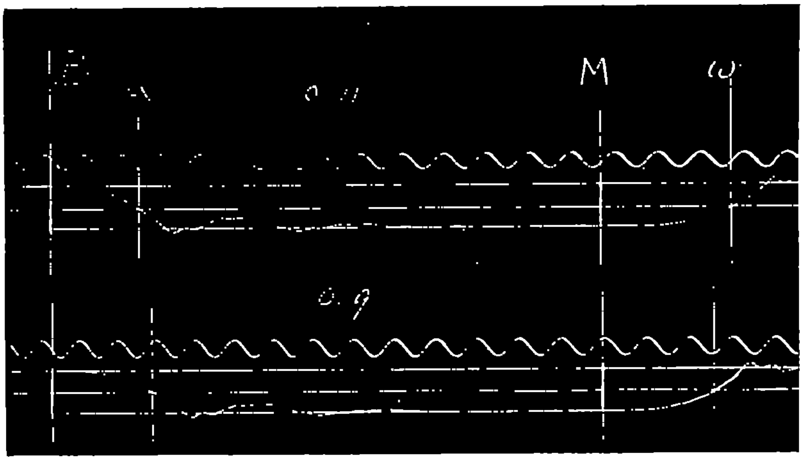


FIG. IV.

Tracings of armature movements, to show effect of varying spring tension. Hands running during interruption of current. Upper spring at zero in both tracings. Lower spring at 11 in upper, at 9 in lower tracing. Letters as in Figs. II and III. Tuning-fork, 200 per sec.

the hands run during interruption of current, the effects (on them) of make and break are reversed. Their latency at make is now that of their arrest, their latency at break that of their start. Therefore, diminishing spring tension (or increasing current) will now start them later, and stop them sooner, and so the reading will be abbreviated. We have not found, as did Ach, that these changes influence the latency at make to a greater extent than at break. This point, however, is of subordinate importance.

All our records show that, both at make and at break, the curvature of the armature tracing is considerable. At make the velocity of the armature is momentarily diminished by the mechanical inertia of the moving parts, and by the make extra current retarding the growth of magnetism. That is to say, with a given spring tension the make curve depends, in part, on the rate of growth of the chronoscope current which is given by Helmholtz' equation (15):

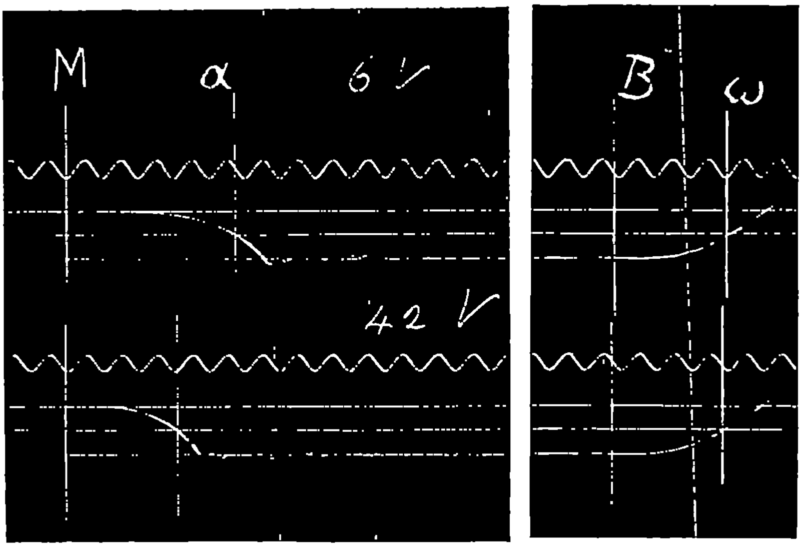


Fig. V.

Tracings of armature movements, to show effect of varying E.M.F. and R. with constant current. Hands running during current flow. Upper spiral spring at 10, lower at 0, current = 80 m.a. (by ammeter) in both cases. The upper tracing was taken with E.M.F. = 6 volts, the lower with 42 volts (by voltmeter); the resistances were therefore 75 and 525 ohms respectively. Letters as in preceding Figs. Tuning-fork, 200 per sec.

$$C_t = \frac{V}{R}\left(1 - e^{-\frac{Rt}{L}}\right),$$

where C_t denotes the intensity of the current t seconds after make; V is the electromotive force, R = the resistance, L = the coefficient of self-induction of the circuit; and e is Napier's base.

The product of $\frac{V}{R}$ into the second term in the bracket represents the intensity (t seconds after make) of the make extra current; and is smaller when R is greater, even when the quotient $\frac{V}{R}$ (the final intensity of the chronoscope current) is constant. Therefore, for a given final intensity of the chronoscope current, the growth rate of the current (and consequently of the magnetism) increases with increasing electromotive force and resistance. This fact, which Hipp(16) empirically discovered for himself, and is known to physicists generally, seems to have escaped the notice of all users of the chronoscope.

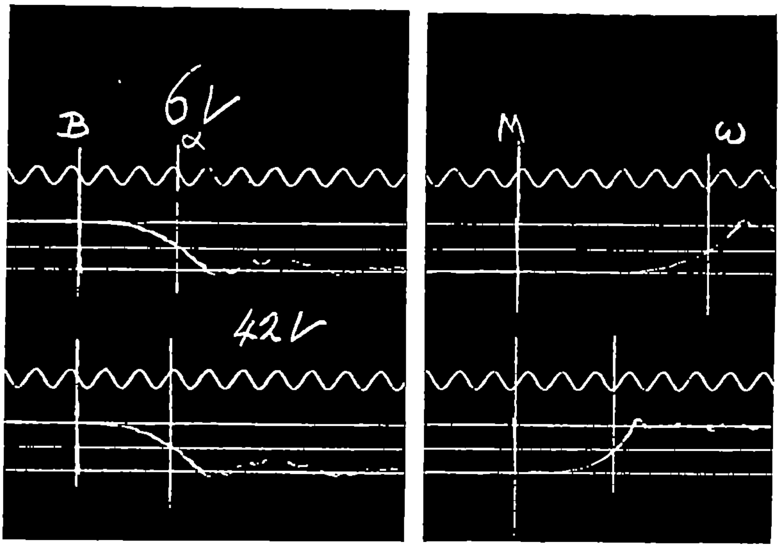


FIG. VI.

Tracings of armature movements, to show effect of varying E.M.F. and R. with constant current. Hands running during interruption of current. Upper spiral spring at 0, lower at 9, current=80 m.a. (by ammeter) in both cases. The upper tracing was taken with E.M.F.=6 volts (by voltmeter), the lower with 42 volts; the resistances were therefore 75 and 525 ohms respectively. Letters as in preceding Figs. Tuning-fork, 200 per sec.

It follows from the above that the make latency of the chronoscope must depend on the resistance in its circuit, apart from the intensity of its current and the tension in the opposing spring. The practical importance of this is shown in Figures V and VI.

At break the conditions are different. When the chronoscope current is completely and 'cleanly' broken[1], no break extra current flows; hence the break curve, and break latency, with a given current, and spring tension, are independent of the resistance in the circuit.

Thus, since the resistance in its circuit influences *per se* the make latency of the chronoscope, without influencing the break latency, the chronoscope readings will depend not merely on current intensity and spring tension, but also on resistance apart from current. With hands running during current flow, a current of given intensity will start them sooner, and so lead to longer readings, when drawn from higher E.M.F. through greater resistance; whilst when hands run during interruption of current, higher E.M.F. and resistance will, *ceteris paribus*, produce earlier arrest, and therefore shorter readings. The influence of the E.M.F. of the chronoscope current on the readings is further shown in Figs. X and XI and in Tables IV and V.

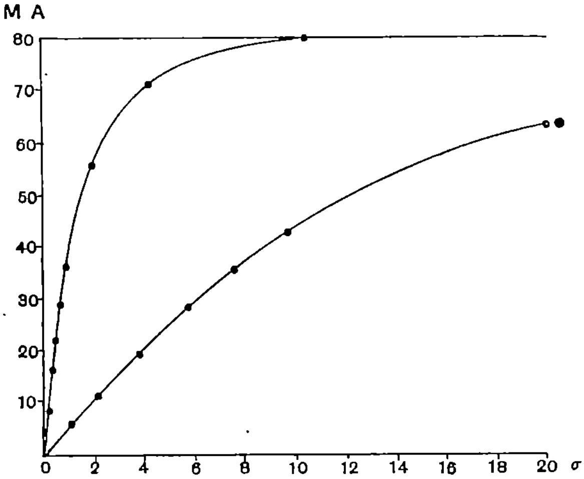


FIG. VII.

Growth of current of 80 m.a. in magnets of chronoscope A ($L = {\cdot}9$ h.). In the upper curve from 42 volts through 525 ohms, in the lower from 6 volts through 75 ohms. Ordinates show current in milliamperes, abscissa gives time in thousandths of a second.

To return to the make and break curves of the armature. The make curve is conditioned by the growth rate of the current and by mechanical inertia. The former of these can be calculated from Helmholtz' equation, when voltage, resistance, and inductance are known. By Lord Rayleigh's method, we found the inductance of the magnets in chronoscope A to be ·9 henry[1]. With the upper spiral spring at 10, it required a current of 45 m.a. to just start the hands. With a current of final intensity = 80 m.a. drawn from 42 volts through 525 ohms, this value (45 m.a.) is reached in 1·4σ (cf. Fig. VII); whilst with the same current drawn from 6 volts through 75 ohms, it is not attained until 10 σ after make. According to this calculation, the hands start 8·6σ later in the second case than in the first, and reference to Fig. V, in which the above conditions were experimentally realised, shows the measured difference between their make latencies in the two cases to be in good agreement with this. Subtracting the electrical latencies (1·4σ and 10σ respectively) from the total make latencies of Fig. V, 13—14σ remain as the latency due to inertia at the spring tension in question.

---

[1] Normally, the break of the chronoscope current is as a fact 'clean' (see Figs. V and VI). With current of excessive intensity, however, and a dusty key, it is possible for a break extra current to pass by spark, and so to affect the break latency.

Admitting the lag of magnetism behind current, it is clear from these figures that mechanical inertia is the dominant factor in the make latency of the chronoscope.

The break curve is not influenced by extra current. Its determining factors are mechanical inertia, and remanent magnetism. We have obtained curves free from the latter in chronoscopes A and B, by the following procedure.

The armature was held down by a hair attached to the lever above, stretched tight, and clamped below. The hair was severed (thus releasing the armature) by a knife rotating with the drum. Under these circumstances the latency of arrest of the chronoscope hands was of course less than when the armature (electrically depressed) was released by break of current. The details are tabulated below.

TABLE I.

Latency of arrest of Chronoscope Hands by upper spiral spring at 10.

	Armature released by severance of hair				Armature released by break of current (80 m.a.)		
	No. of observations	M.	M.V.		No. of observations	M.	M.V.
Chron. A.	6	7·8 σ	·8 σ	...	4	18·6 σ	[illegible]
Chron. B.	8	10·6 σ	·2 σ	...	1	16·5 σ	·4 σ

One such comparison is shown in Fig. VIII.

---

[1] With the armature at its greatest distance from the magnet ur[illegible] mination. The inductance of chronoscopes B and C is about ·5 henry.

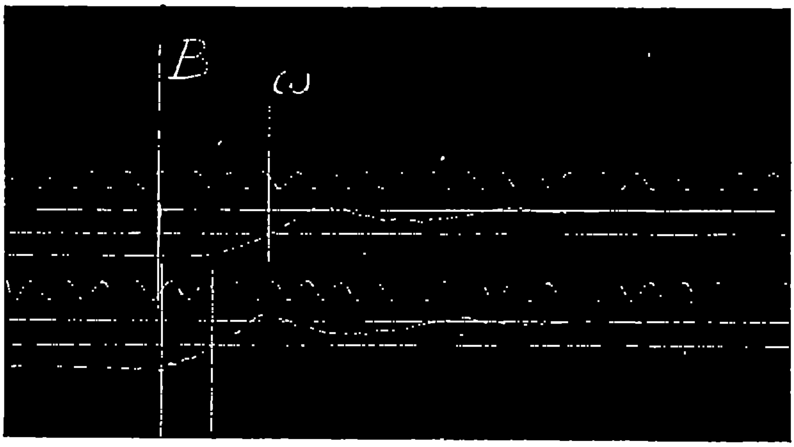


Fig. VIII.

Effect of inertia and remanent magnetism on latency of arrest of hands by upper spiral spring (at 10). In the upper tracing the armature was released by breaking a current of 80 m.a. which flowed round the lower magnets; in the lower tracing by severance of hair. Letters as in preceding Figs. Tuning-fork, 200 per sec. Chr. A.

Thus the effect of the mechanical factor alone was about 6σ less than that of the combined mechanical and magnetic factors, *i.e.* remanent magnetism was responsible for 6σ of the break latencies in the cases in point. The current intensity here was high (if not excessive); mechanical inertia is therefore the dominant factor in the break (as well as in the make) latency of the chronoscope. It is further conspicuous in the prolonged vibration of the armature, at the end of its excursion, seen in all our tracings.

Remanent magnetism increases, of course, with increasing intensity of current. The degree of this increase appears in Fig. IX. It does not, however, increase materially with increasing duration of current, as is shown by the following table.

TABLE II.

Hands running during current flow.

Current Intensity	Current Duration	Upper Spring	Break Latency		
76 m.a.	55 σ	11	13 σ		M. = 13·8 σ M.V. = ·3 σ
,,	5,000	,,	14	(3 readings)	
,,	10,000	,,	14		
,,	15,000	,,	14		
78 m.a.	10,000	10	17		M. = 17·15 σ M.V. = ·4 σ
,,	20,000	,,	17·5		
[illegible]	30,000	,,	18		
[illegible]	40,000	,,	16·5		
,,	50,000	,,	17		
,,	60,000	,,	17·5		

,,
,,

[illegible]urements for periods up to 10,000σ were of like order.

Ach[illegible]

In the lower portion of the above table (current = 78 m.a.), the break latencies are about 4σ longer than when measured under similar circumstances some weeks earlier. This is due to elastic fatigue of the spiral spring, a point which is discussed later (p. 75).

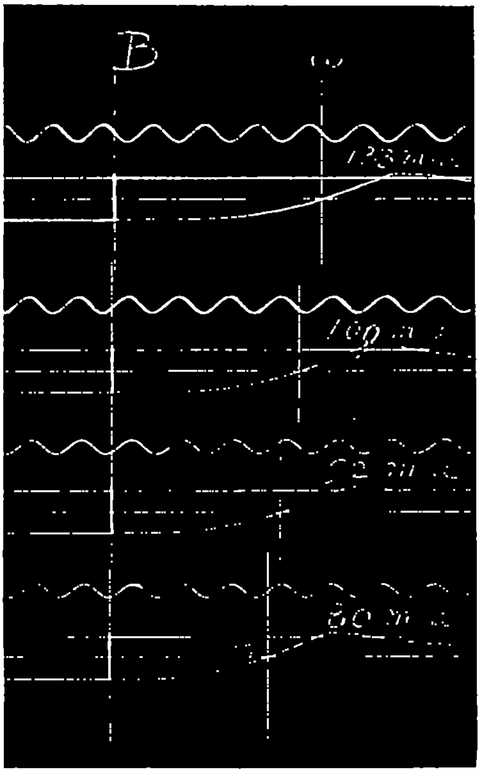


FIG. IX.

Influence of current intensity on effect of remanent magnetism. Latency of arrest of hands by upper spiral spring (at 9) on breaking current. Intensity of current marked against each tracing. Letters as in preceding Figs. Tuning-fork, 200 per sec.

We learn then from the armature lever—

(1) That variation in current intensity or spring tension affects both latencies of the chronoscope (confirmatory of Ach).

(2) That with a given final intensity of current, variation in the resistance of the chronoscope circuit (and therefore in the E.M.F. which furnishes the current) affects the make latency, but not the latency at break. Increasing E.M.F. shortens make latency, and therefore prolongs or shortens the readings.

(3) That mechanical inertia is the greatest factor[1] in the latencies in question.

A further use for the lever is described later (p. 82).

B. **Observations with Wundt's hammer.**

The second series of observations was made by the method employed by Külpe and Kirschmann, viz. that of taking series of chronoscope readings of periods of known duration, furnished by a Wundt's control hammer, and determining the hammer time graphically. For this latter purpose we used a Kronecker-Pfeil electro-magnetic signal, and a tuning-fork vibrating 200 times per second.

The Hammer.

Of the hammer there is little to be said. To diminish noise and vibration it was bolted, with substantial washers, on a heavy slate topped table, apart from the remainder of the apparatus. It was found desirable to readjust its keys whenever their electrical connexions were changed, and to verify their working chronographically before proceeding to test the chronoscope. With these precautions the mean variation of a series of chronographic records of the hammer time (*i.e.* the combined m.v. of chronograph and hammer) is about ·3 σ. Of so small a variation a large proportion is necessarily due to variation in reading the tracings, and there is consequently no material inaccuracy in taking the m.v. of the hammer as zero, and in assigning all the variations in chronoscopic readings of hammer time, to the chronoscope itself. The hammer current was reversed after each fall.

The Chronograph.

With chronograph as with chronoscope, it is obvious that accuracy of reading depends on equality of the make and break latencies of the instrument. A current of 400 m.a. sufficed to ensure this condition, and to give tracings of sufficient amplitude to be readily readable. Both latencies, with this current and with appropriate distance between armature and poles, are less than 1 σ. As with the chronoscope also, the resistance in the chronograph circuit *per se* influences the chronographic readings, but in smaller degree. The inductance[1] of the Pfeil is much smaller than that of the Hipp, hence a given current grows more rapidly in the former than in the latter. Further, the spring of the Pfeil is stiffer than those of the Hipp, hence with the former, currents of higher final intensity can be used to shorten the make latency without sensibly increasing the latency at break. And lastly, the extreme lightness of its moving parts reduces the mechanical inertia of the Pfeil almost to zero, thus accounting largely for the short latencies quoted above. For these reasons the actual duration of the make latency of the Pfeil, with the above current, is practically the same when the current is drawn from 42 volts as when drawn from 6 volts. But the chronographic curve leaves its abscissa more sharply in the former than in the latter case. Its commencement is therefore more readily and accurately determined when higher E.M.F. is employed.

---

[1] Not necessarily so with gross maladjustment of current and spiral spring.

With the higher E.M.F., the range of current (usable with the Pfeil) is considerable, as appears from the subjoined (consecutive) chronographic readings of a constant period.

Intensity of current, from 42 volts	200,	300,	400,	500,	600,	700,	m.a.
Reading.........	100·5	100·5	100·5	101	100·5	100	σ

The mean of these readings is $100{\cdot}5\,\sigma$, with m. v. $= {\cdot}17\,\sigma$.

With lower E.M.F. the range is not so great.

The chronoscope fitted with a lever is in effect a chronograph, and can be used as such. On a drum provided with automatic keys the true value of the chronoscopic readings can be determined without recourse to the control hammer or analogous instruments. But in the absence of automatic keys (on account of its greater inductance and inertia, and weaker springs) the commencement of its upward and downward movements, on a rapidly moving surface, is less distinct than in the case of the Pfeil. Hence without such keys, it does not lend itself so well to chronography of short periods. But with a slowly moving surface (100 mm. per sec.) its chronographic tracings of long periods (3—5 secs.) can be read with considerable accuracy, and comparison of its chronograms with its chronoscopic readings obtained in this way furnishes a convenient method of determining the error of its clockwork (p. 82).

---

[1] The inductance of our Pfeil is ·014 henries at the usual working distance between armature and poles. We are indebted to the kindness of Prof. W. E. Ayrton for this measurement.

The Escapement.

It is well known that the escapement spring of the chronoscope tends to simulate change of pitch, whereby it gives occasional readings of about half the proper value. This is due to variation in production of overtones. It is always obvious to the ear, and so is a source of annoyance rather than of error in work. It is not sufficiently uniform to be corrected by simply doubling the faulty readings, but its occurrence can be diminished or prevented by attending to the escapement. It is characteristic of such springs (*e.g.* tuning-forks), that their overtones depend on the mode and rate of excitation, and the impact of the escapement wheel on the spring can be varied to a considerable extent. We have dealt with the trouble in chronoscope A, by rotating the escapement spring upwards (*i.e.* clockwise) and moving it forwards, so that it is struck by the teeth of the escapement wheel later than was the case with the makers' adjustment. As a result the escapement spring slopes upwards to the left instead of lying horizontal. By this change, and by appropriate arrangement of the cushion and weight, the trouble has been so far eliminated that we have recognised no more than one false note in a year's work, in the course of which some thousands of readings have been made. The value of the change is further shown by the low mean variation of our readings, and by the small error in the rate of movement of the clockwork (pp. 81, 82).

The Armature Springs.

At break of current the armature is brought back to the position which it occupies in absence of current, by the tension in the opposing spiral spring, at a rate depending on the tension in the spring, and on the intensity and duration of the remanent magnetism. The influence of spring tension has been worked out by Külpe and Kirschmann on the older model of the chronoscope, and on the later by Ach. Its general effect has been discussed above (p. 65). Its degree depends on the current.

For no obvious reason many workers use the chronoscope with both these springs in tension. Since they pull in opposite directions one opposes the other, and the effective factor in moving the armature, at break, is the difference in tension between them. We have worked over these springs in every possible combination (10 readings at each), and find that the chronoscope reading is approximately the same for a given difference of spring tension (as expressed on the scales), however that difference is obtained. That is to say, whether the springs are respectively at *e.g.* 7 and 0, 9 and 2, or 15 and 8, the readings will have substantially the same value[1], with the same mean variation. There is consequently no advantage in stretching the spring which pulls in the same direction as the current. There is on the other hand obvious disadvantage in so doing. The springs are susceptible to fatigue, and their recovery, though for the moment apparently complete, is in the long run imperfect. This fatigue appears earlier and is more pronounced when tension is high; hence the disadvantage of using unnecessary tension by working with both springs stretched. It shows itself in two ways. Firstly, the value of the readings changes in the same direction as it would if spring tension were reduced; and secondly, their mean variation is increased. It is especially noticeable in chronoscope A, which has been somewhat severely handled during the last fifteen months, and whose springs are distinctly less elastic and more fatiguable now than a year ago. In lesser degree it is also seen in B and C, but is avoidable in all three instruments by working with moderate spring tension.

The following are instances of its occurrence, both in A.

TABLE III.

Spiral springs 8, 0. Current 70 m.a. from 42 volts. Hands running during current flow. Analysis of 100 consecutive readings.

		Extremes		Mean	M.V.
Readings	1—10	279 σ,	281 σ	279·9 σ	·72 σ
„	11—20	„	„	280·1	·54
„	21—30	„	„	279·7	·70
„	31—40	„	„	279·9	·36
„	41—50	„	„	279·9	·72
„	51—60	„	„	280·1	·54
„	61—70	„	„	280·3	·56
„	71—80	„	„	280·5	·60
„	81—90	„	„	280·5	·60
„	91—100	„	286	282	1·8

Fatigue appears in the last ten readings.

In the second case the springs were at 9 and 0 respectively, and the current was 80 m.a. from 42 volts. The first 20 readings averaged

---

[1] Apart from elastic fatigue.

287·05 σ, with extremes $= 286\ \sigma$ and $288\ \sigma$, and with m.v. $= \cdot 57\ \sigma$. The next 6 averaged $289\ \sigma$, with extremes $= 286\ \sigma$, and $291\ \sigma$, and m.v. $= 1\cdot 3\ \sigma$.

In neither of these cases can the fatigue be said to have produced gross error, considered, *e.g.*, in relation to measurement of reaction time. The tendency, however, is unmistakeable. The final mean variation, though not great in itself, is large in proportion to its initial value, and would presumably have increased, had not the interests of the chronoscope suggested cessation of the test. Further evidence of fatigue is quoted on p. 71.

The Chronoscope Current.

Figures VIII and IX, showing that remanent magnetism may, even against considerable spring tension, delay the movement of the armature to the extent of 6—10 σ, demonstrate that it is present after break in no inconsiderable degree, and raise the question of reversal of current in a series of readings, since remanent magnetism (in absence of reversal) ultimately becomes permanent. Regular reversal appears to be the custom of most workers.

Occasional omission to reverse gives rise to no appreciable irregularity in a series of chronoscopic readings of equal periods. Reversal after a series of omissions is, however, often followed by an anomalous reading; it is therefore desirable to reverse after each reading, or to discard the reading immediately following reversal, when infrequent reversal is the practice. Irregular reversal may lead, like total omission of reversal, to permanent magnetism. This, when of small degree, does not add greatly to the mean variation of the readings, as we found with chronoscope B. The upper magnet here was so far permanently magnetised as to show marked polarity when tested by a rough compass needle, and to give regularly alternating higher and lower readings[1] as follows: 302, 300, 301, 300, 302 σ; and with higher spring tension: 304, 305, 303, 304, 302 σ. The mean variation of the first series is $\cdot 80\ \sigma$, that of the second $\cdot 88\ \sigma$, and such variations are small.

To avoid this condition, and to prevent its increase when present, regular as well as frequent reversal is necessary, and our own practice with chronoscope, as with chronograph, is to reverse current after reading. Association between pulling the stopping string of the chronoscope and turning the commutator is readily established, and it is less effort to reverse thus, after every reading, than to do so after (say) each tenth, where no such simple association is available.

---

[1] Current was reversed after each reading.

Ach[1] quotes Sommer as getting "good results" without reversal of current, and himself states that reversal may be positively undesirable (*schädlich*). This statement he bases on the observation that with two of his chronoscopes, a weak current which sufficed to move the armature when it flowed in one direction, failed to do so when reversed. The magnets in question were obviously permanently magnetic, and the response of the armature to the weak current depended on the direction of that current, because in the one case the permanent and electro-magnetisms were of the same, and in the other case of opposite signs, in other words, because the permanent magnetism assisted the electro-magnetism due to the weak current in one case, and opposed it in the other. With stronger currents Ach naturally failed to find this phenomenon.

It appears from p. 66 that the chronoscope current is susceptible of two adjustments, (i) in intensity, (ii) in electromotive force and resistance, apart from intensity.

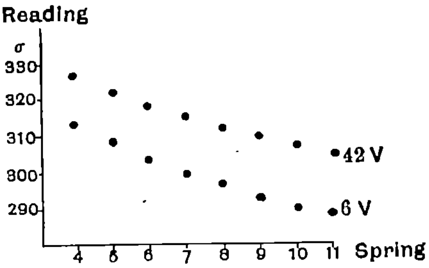


FIG. X.

Chronoscope readings of a constant time interval, with constant current, from 6 volts and 42 volts, respectively. Hands running during current flow. Lower spring at 0, tension of upper spring marked along abscissa. Current 81 m.a. in both cases; from 42 volts in upper, from 6 volts in lower curve. Higher E.M.F. produces higher readings, with flatter gradient in the curve.

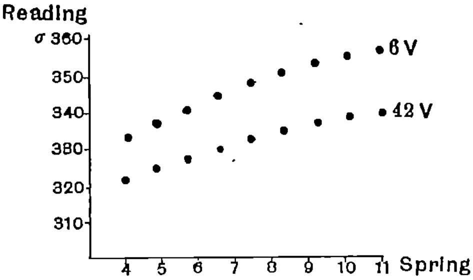


FIG. XI.

Chronoscope readings of a constant time interval, with constant current from 6 volts and 42 volts, respectively. Hands running during interruption of current. Upper spring at 0, lower spring tension marked along abscissa. Current 78 m.a. in both cases; from 6 volts in upper, from 42 volts in lower curve. Higher E.M.F. leads to lower readings, with flatter gradient in the curve.

With a given spring tension, increasing intensity leads to higher or lower readings, according as the hands run during current-flow, or during interruption of current. Since spring tension and current intensity affect the readings in opposite directions, the chronoscope will (to give true readings) require higher spring tension when the current is stronger. Hence unnecessarily high intensity is undesirable, in the interest of the springs, as well as in that of the magnets. Further, the effect on the readings of varying spring tension decreases with increasing current intensity.

---

[1] *Loc. cit.* p. 291.

Increasing electromotive force and resistance in the same ratio (*i.e.* current intensity unaltered) affects the readings in the same direction as increasing current intensity (pp. 66—68). This is shown also in Figs. V and VI. Up to a certain point there is gain in uniformity by drawing current from high electromotive force through high resistance, as is shown by the following tables of mean variations.

TABLE IV.

Mean variation of readings, with current from different pressures. Hands running during current flow.

Upper Spring......		6	7	8	9	10	11	12	13
		σ	σ	σ	σ	σ	σ	σ	σ
Current 76 m.a. from 4·2 volts	M.V. =	·80	1·0	1·0	1·24	1·3	1·1	1·4	1·5
Current 78 m.a. from 30 volts	M.V. =	·62	·68	·20	·48	·42	·48	·48	·54
Current 80 m.a. from 111 volts	M.V. =	—	—	·54	·56	·58	·48	0·0	·64

Ten readings were taken at each spring tension.

TABLE V.

Mean variation of readings, with current from different pressures. Hands running during interruption of current.

Lower Spring......		4	5	6	7	8	9	10
		σ	σ	σ	σ	σ	σ	σ
Current 80 m.a. from 5·6 volts	M.V. =	1·2	1·32	1·36	·84	1·90	1·8	1·8
Current 78 m.a. from 42 volts	M.V. =	1·04	1·04	1·2	0·0	·40	·32	1·28
Current 80 m.a. from 111 volts	M.V. =	1·6	·92	·76	·72	·90	·60	·88

Ten readings were taken at each spring tension in the first and third lines, five readings in the second.

These tables show smaller variations where higher electromotive force was employed. There is no obvious gain however in exceeding 30—40 volts.

It has occasionally happened that with low pressure (6 volts), and hands running during current flow, we have obtained as good readings as with 30—40 volts. But on the whole the higher pressure has proved the more reliable, particularly when hands run during interruption of current; we have therefore adopted a battery of twenty small accumulators[1] (=40—42 volts) as our routine source of current, both for chronoscopic and chronographic purposes.

From the above observations with chronoscope and hammer we infer—

1. That the mean variation of the hammer can be reduced to negligible proportions.

2. That the 'octave' error of the chronoscope can be eliminated.

3. That elastic fatigue and permanent magnetism are factors to be reckoned with.

4. That the E. M. F., apart from the intensity of the chronoscope current, influences the constancy as well as the magnitude of the readings.

PART III. THE ACCURACY AND CONTROL OF THE CHRONOSCOPE.

The value of the chronoscope as a working mechanism depends, in the main, on four points.

1. On its accuracy and constancy in reading periods of equal duration.

2. On the proportionality of its readings to the periods they represent, when these periods vary.

3. On the range of its readings.

4. On its durability.

In order that its readings may be accurate, it is necessary that the make and break latencies of the hands shall be equal (Cattell), and that the error of its clockwork shall be negligible (Müller and Schumann).

---

[1] This battery occupies no more than 56 x 15 cm. of the work table.

The latencies depend on appropriate adjustment of armature springs and current (see above, p. 65), and any disparity between them leads to error in reading, which is constant for all intervals. This error may be determined with control hammer and chronograph, or with chronoscope lever and drum fitted with automatic keys, using in either case a short period, so that any small error in clockwork may be negligible. When determined, it is readily reduced to 1 σ or less by readjustment of the spring. For example: Chronoscope A recorded such an interval, as being 85 σ. Attempt to change the reading to 80 σ yielded, at the first readjustment, 80·8 σ (mean of five readings with m.v. = ·32 σ); and further attempt to change the reading to 90 σ yielded equally immediately 89·6 σ, the mean of five readings with m.v. = ·48 σ. In the first case, more detailed readjustment would have led to still closer approximation to the desired reading. This error can therefore be eliminated for all practical purposes.

As a criterion of constancy we have usually taken the mean variation of ten consecutive readings, more rarely that of five, and as an extreme test have occasionally resorted to analysis of one hundred such. Of the last we quote the following.

TABLE VI.

Analysis of 100 *consecutive readings, grouped in tens. Hands running during interruption of current. Current* 140–150 *m.a. from* 8 *volts. Upper spring* 11, *lower spring* 12.

		Extremes		Mean	M.V.	Successive means of Groups
		σ	σ	σ	σ	σ
Readings	1—10	251,	257	253·5	1·20	
,,	11—20	250,	256	253·3	1·76	253·4
,,	21—30	249,	256	253·1	1·50	253·3
,,	31—40	251,	257	253·9	2·12	253·5
,,	41—50	250,	256	253·3	1·84	253·4
,,	51—60	250,	255	252·8	1·48	253·3
,,	61—70	250,	258	253·2	1·84	253·3
,,	71—80	252,	256	254·8	0·98	253·5
,,	81—90	253,	259	255·1	1·74	253·7
,,	91—100	252,	258	255·0	2·0	253·8

This table illustrates the constancy obtainable with gross maladjustment. The current was at least of double the necessary intensity, the spring tension (difference) $\frac{1}{4}$—$\frac{1}{8}$ of the optimum for the current. The electromotive force also was undesirably low.

TABLE VII.

Analysis of 100 *consecutive readings, grouped in tens. Hands running during interruption of current. Current* 78 *m.a. from* 42 *volts. Upper spring* 0, *lower spring* 9.

		Extremes		Mean	M.V.	Successive means of Groups
		σ	σ	σ	σ	σ
Readings	1—10	335,	338	336·0	0·80	
,,	11—20	334,	337	335·6	0·80	335·8
,,	21—30	334,	337	335·6	0·80	335·7
,,	31—40	334,	338	336·5	1·00	335·9
,,	41—50	335,	339	336·5	0·80	336·0
,,	51—60	335,	338	336·2	0·84	336·1
,,	61—70	336,	340	337·2	1·20	336·2
,,	71—80	335,	338	336·8	0·68	336·3
,,	81—90	335,	338	336·5	0·90	336·3
,,	91—100	334,	338	336·6	0·76	336·4

This table evidences better adjustment. The extreme readings for the whole series differ by 6 σ, and the average mean variation is ·86 σ, *i.e.* < 1 σ.

TABLE VIII.

Analysis of 100 *consecutive readings, grouped in tens. Hands running during current flow. Current* 50 *m.a. from* 42 *volts. Upper spring* 5, *lower spring* 0.

		Extremes		Mean	M.V.	Successive means of Groups
		σ	σ	σ	σ	σ
Readings	1—10	286,	288	286·8	0·64	
,,	11—20	286,	288	286·7	0·56	286·8
,,	21—30	286,	288	287·0	0·80	286·8
,,	31—40	286,	288	287·2	0·48	286·9
,,	41—50	287,	288	287·4	0·48	287·0
,,	51—60	287,	288	287·3	0·42	287·1
,,	61—70	286,	288	287·2	0·48	287·1
,,	71—80	287,	288	287·3	0·42	287·1
,,	81—90	287,	288	287·3	0·42	287·1
,,	91—100	286,	288	287·0	0·40	287·1

Comparison of Tables VII and VIII shows the mean variation when hands run during current flow to be markedly less than it is when hands run during interruption of current. This has been consistently the case throughout our work. In Table VIII the mean readings in the third column are (to the nearest sigma) 287 σ throughout, and no single reading differed by more than 1 σ from this. In the hundred readings

286 σ occurred 16 times, 287 σ—56 times, and 288 σ—28 times. For practical purposes therefore every reading was significant. The columns of successive means, in Tables VI—VIII, indicate that at most ten readings are necessary to determine the constant error of the Hipp adjustments, whilst the mean variations show that in the absence of excessive spring tension constancy is maintained in a long series of readings.

Within the limits of Wundt's hammer we have not found the variation of chronoscope readings to increase noticeably with increasing duration of the periods read. The variations are therefore chiefly due to the starting and stopping mechanism, not to the clockwork.

We have made measurements of the ratio of the Hipp readings to the periods they represent, *i.e.* measurements of the clockwork error as follows.

The springs were adjusted as above (p. 80) to eliminate the constant error. Graphic records of the armature movements (see pp. 63, 74) of various periods up to 4 or 5 secs., obtained by closing and opening an ordinary spring key, were then taken on a drum surface moving about 100 mm. per sec. A tuning-fork (100 per second) provided the time tracing. The graphic values of the periods, compared with the simultaneous chronoscope readings, yielded the subjoined results.

CHRONOSCOPE A.

Preliminary test of adjustments with hammer and chronograph.

Chronoscope Readings (10)		Chronograph Readings (7)	
M.	M.V.	M.	M.V.
82·8 σ	·76 σ	82·7 σ	·80 σ

The adjustments were therefore accurate.

TABLE IX.

Chronoscope used as chronograph.

Chronoscopic Reading	Chronographic Reading	Chronoscopic Reading / Chronographic Reading
766 σ	768 σ	0·9972
1152	1152	1·0000
1831	1834	0·9984
1891	1893	0·9988
5019	5027	0·9984
5547	5556	0·9984

The ratio of each pair of readings is given in the third column for the purpose of showing the uniformity in the series of comparisons. The difference between the highest and lowest ratios is 0·0028, and the mean of the series is 0·9985, with m.v. = 0·0006.

In practice it is less troublesome, as well as more accurate, to determine the ratio:—$\frac{\text{sum of chronoscopic readings}}{\text{sum of chronographic readings}}$.

In the present case this ratio = ·9984, which indicates that the clockwork runs ·16 °/₀ too slowly. Tables VII, VIII, and IX show the value of the readjustment of the escapement described on p. 74.

Chronoscope B.

Preliminary test of adjustments with hammer and chronograph.

Chronoscope Readings (10)		Chronograph Readings (5)	
M.	M.V.	M.	M.V.
92·3 σ	·70 σ	93·3 σ	·36 σ

The adjustments were therefore sufficiently accurate.

TABLE X.

Chronoscope used as chronograph.

Chronoscopic Reading	Chronographic Reading	$\frac{\text{Chronoscopic Reading}}{\text{Chronographic Reading}}$
723 σ	718 σ	1·007
1340	1332	1·006
1415	1405	1·007
2001	1981	1·010
2451	2428	1·009
3211	3176	1·011
3441	3410	1·009
3736	3708	1·008
3918	3885	1·008

The difference between the highest and lowest ratios in this case is 0·004, and the mean variation = 0·001. The ratio

$$\frac{\text{sum of chronoscopic readings}}{\text{sum of chronographic readings}} = 1{\cdot}008.$$

The clockwork therefore runs ·8 °/₀ too fast, and correction of this small error is well within the range of the weight provided in the escapement for the purpose.

The above method of measuring the clockwork error is simple, convenient, and accurate. Almost the whole of the error arises in estimating the fractions of the tuning-fork waves at the beginning and end of the graphic records. And since these errors are not more likely to occur in either one direction than in the opposite, they tend to disappear in the sum of the graphic readings.

It is noteworthy that in Tables IX and X the mean error of the clockwork is shown exactly by the individual ratios of the highest readings. Four or five such comparisons, of intervals of 4—5 seconds, therefore suffice to determine the error in question.

The range of periods that can be read with the chronoscope is wide. Its upper limit depends solely on the time of fall of the weight (about 70 secs. in chr. A). When hands run during interruption of current, this is *à priori* obvious; and in the other case, *i.e.* when hands run during current flow, its truth is demonstrated in Table II, which shows that the break latency of the hands does not increase materially with increasing current duration, up to one minute.

The lower limit is, as stated by Ach, about 30 σ. It is therefore far less than the shortest reaction time. It appears to be largely determined by the inertia of the armature. Our tracings show that this vibrates for 30 σ (or more) after it reaches the position to which it is brought by make or break of current. Hence the limit in question must differ somewhat in different chronoscopes. Another important factor is the rate of growth of the current (pp. 66, 68). This is illustrated by the following readings, all obtained with chronoscope A ($L = \cdot 9h$).

TABLE XI.

Hands running during interruption of current. Chronographic value of period = 29·2 σ with M.V. = ·24 σ.

Current		Chronoscopic Readings	
Intensity	E.M.F.	Mean (of 10)	M.V.
80 m.a.	6 volts	51·5 σ	2·40 σ
,,	42 ,,	32·7	1·56
,,	112 ,,	30·7	1·42

In this table the latency of start of the hands was about 14 σ in all three cases. During their movement, therefore, the armature was never at rest. Hence the high mean variations throughout, and particularly in the first case, where arrest was effected by the most slowly growing current (p. 68).

TABLE XII.

Hands running during current flow. Chronographic value of period $= 28{\cdot}8\sigma$ *with* M.V. $= {\cdot}28\sigma$.

Current		Chronoscopic Readings	
Intensity	E. M. F.	Mean (of 10)	M. V.
80 m.a.	6 volts	18·0 σ	1·8 σ
,,	42 ,,	28·8	1·16
,,	112 ,,	28·9	0·88

Here again the armature was vibrating all the time during which the hands were running. Further, in the first case the current never attained its nominal value. As late as 30σ after make it would only have reached 73—74 m.a. (p. 66).

Our knowledge of the durability of the chronoscope rests on the history of A and C.

A was purchased in June, 1904, has been incessantly in work, and further subjected to the severe handling which yielded the basis of this paper. The only sign of failure is seen in the diminished elasticity of the spiral springs, and these are still (March 1906) efficient when used in moderate tension (see Table VIII). C was purchased in 1884, and has been in continuous use since 1893. As compared with A, its variations are slightly higher, but lower than those recorded in the majority of papers on the chronoscope.

GENERAL CONCLUSIONS.

As to employment and control of the chronoscope, we draw the following conclusions:

The errors due to irregularity in overtones, and to slow (or fast) running, may be practically eliminated by proper adjustment of the escapement. This done, the error of the chronoscope is constant, for readings of 30—60,000 σ.

The armature springs should not be simultaneously in tension. The one in use should not be stretched beyond 5°—10° of its scale, and should be zeroed at the end of each day's work. It can readily be adjusted to reduce the constant error of the chronoscope to less than 1σ. Hence the chronoscope can be made to give 'true readings' of periods from 30σ to 1 min. in duration. The mean variation of its readings is less than 1σ.

The current should be drawn from moderately high E.M.F. (30—40 volts). Its intensity should be such as to enable true readings with the armature spring in moderate tension (see above). For this 50—80 m.a. are sufficient. It should be frequently and regularly reversed.

The method of graphically recording the armature movements, described in the text, measures the error of running of the chronoscope with convenience and accuracy. With the aid of automatic keys it can also be used to measure the constant error of the chronoscope to 1σ or less, and in the absence of such keys to $3—4\sigma$. For rapidly eliminating this error, Wundt's hammer is more convenient, and does not materially contribute to the variations of the readings which it helps to furnish.

The chronoscope, in accuracy and durability, is a thoroughly reliable instrument, having a range more than sufficient for reaction time measurements.

In conclusion, we desire to express our cordial thanks to Dr A. D. Waller for the resources of his laboratory.

APPENDIX.

NOTE ON A MODEL OF WHEATSTONE'S CHRONOSCOPE.

We are indebted to Prof. H. A. Wilson for the loan of a Wheatstone Chronoscope, from the Wheatstone Museum of King's College, London.

Its general appearance is shown in Fig. XII. The dial is provided with two concentric hands, and reads to thousandths of a second. Of the three keys,—one serves to wind up the clock, a second to 'zero' the hands, and the third to stop (or start) the clockwork, by arresting (or releasing) the escapement wheel. The hands are not permanently geared to the clockwork, but engaged by the same mechanism ('face-wheel,' 'Kron-rad') as in a Hipp chronoscope. The electro-magnetic details are however different. The hands run only during current-flow. The escapement is a vibrating spring similar to that of Hipp's model, but actuated by a current of air from a bellows (not shown in Fig.), instead of by the escapement wheel.

These points lead one to infer, that the model in question is of later date than that described by Wheatstone in the *Comptes Rendus*: that it is one in which he had adopted the improvements credited to Hipp by Oelschläger, and in which he was trying to eliminate the 'octave error' of Hipp's escapement by the employment of an air driven reed.

This inference is borne out by the presence, amongst the apparatus bequeathed to King's College by Wheatstone, of what, even in the absence of a maker's name, must be regarded as an early model of Hipp's chronoscope, agreeing as it does in every detail with the early pictures and descriptions of that instrument. The substitution of a bellows driven reed for Hipp's mechanically excited spring, was a natural outcome of Wheatstone's association with the English concertina. Unfortunately the age of the apparatus renders worthless all attempts to gauge its efficiency.

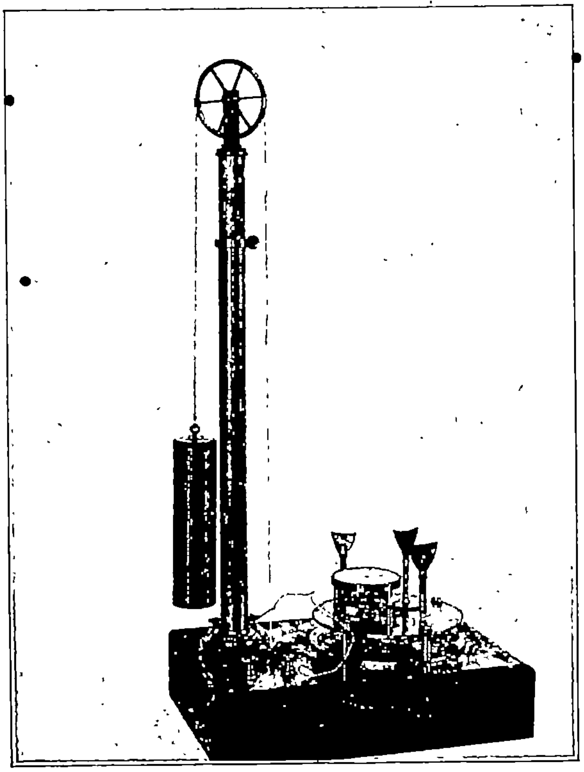


FIG. XII.

BIBLIOGRAPHY.

1. Sir C. Wheatstone, *Académie des Sciences, Comptes Rendus*, T. XX. p. 1554. Cf. C. Kuhn, *Handbuch der Angewandten Elektricitätslehre*, SS. 1175, 1185.
2. W. Eisenlohr, *Physik*, 7te Aufl. 1856, Th. II. S. 627.
3. W. Oelschläger, *Polytechnisches Journal*, Bd. CX. S. 184, and *Poggendorf's Annalen*, 1848, Bd. LXXIV. S. 589.
   W. Oelschläger, *Polytechnisches Journal*, Bd. CXIV. S. 255.
4. A. Hirsch, *Moleschotts Untersuchungen zur Naturlehre des Menschen u. d. Thiere*, Bd. IX. S. 188.
5. J. Merkel, *Philosophische Studien*, Bd. II. S. 85.
6. G. O. Berger, *Phil. Stud.*, Bd. III. S. 35.
7. J. McKeen Cattell, *Phil. Stud.*, Bd. III. S. 305. Cf. *Phil. Stud.*, Bd. IX. S. 307; *American Journal of Psychology*, Vol. IV. p. 596.
8. L. Lange, *Phil. Stud.*, Bd. IV. SS. 458, 483.
9. G. E. Müller, *Göttingische Gelehrte Anzeigen*, 1891, p. 393. Cf. *Zeitsch. für Psych. u. Phys. der Sinnesorgane*, Bd. IV. S. 404.
10. E. Kraepelin, *Ueber die Beeinflussung einfacher psychischen Vorgänge.*
11. Külpe u. Kirschmann, *Phil. Stud.*, Bd. VIII. S. 145.
12. R. Sommer, *Lehrbuch der psychopathologischen Untersuchungsmethoden*, 1899.
13. Müller u. Schumann, *Zeitsch. für Psych. u. Phys. der Sinnesorgane.* Ergänzungsband I. Anhang. Cf. Schumann, *op. cit.*, Bd. XVII. S. 253.
14. N. Ach, *Über die Willenstätigkeit u. das Denken, mit einem Anhange über das Hipp'sche Chronoskop.* Göttingen, 1905.
15. H. v. Helmholtz, *Poggendorf's Annalen*, 1851, Bd. LXXXIII. S. 505.
16. M. Hipp, *Mittheil. Bern. Naturforsch. Gesellsch.*, 1855, S. 190.

'FOOTRULE' FOR MEASURING CORRELATION.

By C. SPEARMAN.

§ 1. *Need of a very easy, but yet precise, method of measuring correlation. Estimate by intuition is vague and illusive.*

§ 2. *Necessity of comparison by rank, as absolute measurements are not properly comparable with one another.*

§ 3. *Existing methods of calculating correlation. The standard 'r' method. Various methods proposed by mathematicians and investigators respectively.*

§ 4. *Proposed easy 'R' method, with an actual illustration. Usage of the 'probable error.' The two kinds of probable errors.*

§ 5. *Relation of the R to the r method. Latter can be simplified, and then only differs from former in that certain data are squared.* $r = sin\left(\frac{\pi}{2}R\right)$. *Evidence that psychological correlations approximate to one constant form, and therefore are really measurable by a single coefficient. Summary.*

Appendix. Mathematical proofs in support of the R method.

§ 1. *Need of a 'footrule' for correlation.*

NOWADAYS little apology is required for any attempt to improve the psychological methods of dealing with quantity. The investigation of quantity is not intended, as some would seem to imagine, to replace that of quality, but to complement it. Qualitative and quantitative phenomena are, as it were, the flesh and bones of psychology; if this branch of science is meaningless without fine qualitative self-observation, it is at least rickety and crank without a frame-work of hard quantitative facts.

The particular domain of quantitative fact to be considered here is one which has but recently been fully annexed by science, and which seems of an unparalleled extent and richness, only waiting to be exploited. Previously science had almost exclusively devoted itself to regularities or 'laws'; but lately it has been learning to deal even with tendency to regularity, or, as it is called, to measure *the degree of correlation* between any two things.

Now, every one knows that the length of a line may be estimated with very varying grades of exactitude; many purposes are served perfectly well by a single glance of the eye; at other times, on the contrary, we require tediously repeated observations with an elaborate micrometer; while between these two extremes, there are a great number of cases where the object in view is much best met by the simple application, say, of a footrule. In this paper, it is desired to emphasize the fact, that precisely the same holds good in correlation also; mere casual observation is enough, for instance, to reveal the general fact, that warm damp weather has some connection or other with the growth of fungi; on the other hand, certain correspondences between biological events are taxing the utmost resources of statistical mathematics; while here too, in correlation, there is a middle region, where we are much in need of a plain handy 'footrule.'

The want of some such convenient measure is indeed, I believe, largely responsible for the slow progress and innumerable apparent contradictions of psychological research. In the course of almost every investigation, or perusal of investigations, one has many times occasion to compare one series of figures with another, to see whether and how much they vary concurrently. And this comparison is still almost universally performed on the primitive plan of just inspecting the figures and forming a general impression. Hereby the mind is called upon to perform a feat intuitively which it is only fitted to execute discursively. If a person does not happen to be a mathematical prodigy, just looking over the figures will no more suffice for him to determine accurately a correlation, than it will enable him to effect any other similar quantitative operation, say, addition, multiplication, or division. And even if the mind did possess a fine general sense of correlation, the present resources of language could not bring it to expression; such vague generalities as 'large,' 'small,' 'medium,' etc. are far removed from the precise terms required for scientific usefulness.

Nor is this vagueness all, or perhaps even the worst, that is to be said against estimating correlation by the sole light of intuition. This sort of estimate is further exposed to various formidable illusions; dozens of instances could be quoted, where authors have published arrays of figures as being an irrefragable proof of correlation, whereas actual reckoning quickly shows none whatever to exist; and even more frequent is the opposite illusion, which has led many of our best and soundest workers to deny positively any correlation, whereas the reckoning easily establishes one beyond all doubt. Some, but by no means all, of these illusions are dependent upon the manner in which the figures happen to be arranged. For instance, it has frequently occurred, that one author announces little or no correlation between two things, because he has found the ratio between them to vary irregularly from case to case; while another author declares the correlation between the same things to be very high, because he has been dealing with averages, in which of course individual fluctuations cancel one another.

And if the intuitive estimate breaks down when it only has to judge the actual proportionality between any given values, still far more incompetent is it to decide whether any actually observed proportionality should be taken as a *general* fact, and not a mere accidental coincidence between the particular cases given.

§ 2. *Comparison by rank.*

Attention must here be drawn to an important point, which does not seem to have hitherto met with sufficient regard. In order that a precise measure of correlation between two series of values may have full significance, these two species of values should be *properly comparable with one another.*

This requisite does not make itself felt as a difficulty, so long as the two species of values are homogeneous.

In physical and physiological matters this generally occurs, in that both values are usually spatial dimensions; such is the case, for instance, in the correlation between the lengths of the arm and the leg respectively, or in that between the length and breadth of the skull.

But the matter changes as soon as we turn to psychology. Suppose that we wanted to measure the correlation between the skin's 'spatial sense[1]' and its sensitivity to pain; we should measure both senses at a great many different places all over the body; the spatial sense would probably be measured by ascertaining the threshold at which two points could just be distinguished from one another, while the sensitivity to pain might be tested by determining the amount of pressure that just begins to be painful. But then arises the obvious question, how far are the variations of distance between the two points of the aesthesiometer legitimately comparable with the variations of pressure of the algometer? Unquestionably, indeed, the varying distance between the points is some mathematical function of the varying spatial sense, and similarly as regards the pressure and the sensitivity to pain; but we can never hope to determine such a function, nor even to show that it is the same in both cases[1].

---

[1] The German 'Raumsinn'.

All that we can properly assert seems to be that a greater distance between the points implies a duller spatial sense, and a stronger pressure indicates less sensitivity to pain. On this assumption it is clear that, if the two senses were perfectly correlated with one another, that part of the skin which required the least distance between the points would also need the smallest pressure; the part that wanted the second smallest distance would also want the second smallest pressure, and so on. But when we confine ourselves to this sole legitimate assumption, we are doing neither more nor less than converting the experimentally obtained measurements from absolute quantities into *comparative ranks.*

The disadvantage of conversion into rank is that thereby a certain amount of the experimental information is sacrificed. We merely call *A* one rank higher than *B* and *B* one higher than *C*, although we may actually know that the intervals *A*—*B* and *B*—*C* are of different magnitudes. But the amount of sacrifice is only of the same order as occurs in the very general procedure of grouping the cases into classes; here the insignificant difference between the lowest value in one class and the highest value in the class just beneath is handled in quite the same way as the considerable difference between the highest value in the higher class and the lowest in the lower class. True enough, in grouping by classes the extent of any single such injustice cannot possibly exceed the range of two classes, whereas in using ranks there is no limit. But this point of view is based upon a wrong principle; in the matter of correlation we have not to consider the conceivable vagaries of individual cases, but the amount of disturbance actually to be feared according to the laws of probability in the finally deduced coefficient. From this truer point of view, theoretically it would not be hard to demonstrate that the sacrifice of information by using ranks must really be as insignificant as that by grouping into classes; and practically, experience has convinced me that the results obtained by using ranks, instead of being less constant and reliable, are on the contrary (owing to certain other moments[2]) even more so.

[1] Obviously, instead of obtaining 'thresholds,' we might from the same experimental data equally well derive 'obtuseness values' ('Stumpfheitswerte'), as was done in all extensive investigations of the Vierordt school. Thereupon all the proportions, and with these all the correlations, would become altered.

[2] Cf. *Amer. J. Psych.* 1904, Vol. XIII. pp. 79–82.

§ 3. *Existing methods of calculating correlation.*

Let us now consider for a moment how far our requirements are met by any of the methods of calculating correlation that are already known.

The present, as one may fairly call it, 'standard' method is the one chiefly elaborated by Bravais, Galton and Pearson. The total degree of proportionality between the two compared series of values is summed up in a numerical coefficient, symbolized by the letter r. When the proportionality is complete throughout, $r = 1$; the more incomplete the general proportionality, the smaller becomes r; and when there is no tendency whatever to proportionality, $r = 0$. A further formula gives the 'probable error,' showing just how much the value of r may reasonably be expected to be disturbed by mere chance.

There is not the least difficulty in applying this r method to correlations by rank. I have worked out about 40 coefficients, first with absolute measurements, and then again after converting these into relative ranks. The mean value of correlation reckoned in the two respective ways turned out to be identical up to the 3rd decimal place inclusive. Of course under special circumstances the values arrived at in the two different ways might differ to a considerable extent; but then, in accordance with the preceding section, we must consider the value given by means of ranks as being the truer of the two; ranks will always express perfect correlation by $r = 1$, as intended by the formula; whereas absolute measurements, if heterogeneous, will usually *not* do so. Even the formula for the probable error does not seem to become appreciably inaccurate when ranks are used, except in the case of a very short series and a very high correlation (say, over 0·90), when the value given by the formula becomes much too small.

The great objection to this r method is that it involves very laborious calculations. Despite all other merits, this quite unfits it for the 'footrule,' of which we are now in quest.

The same objection applies, of course, even more forcibly to the ingenious complications introduced by Pearson, in order to measure 'skew' correlation.

Some continental mathematicians have recently endeavoured to express correlations more accurately by means of a series of terms, instead of a single one. Not only is the calculation more tedious than ever, but even the results are of very doubtful serviceability in actual research.

Most of the other hitherto proposed methods of reckoning correlation have been devised on emergency in the course of particular investigations. These rarely at all answer the needs of a scientific method. Even when the correlation has been summed up in a definite expression, this has usually had no upper limit marking perfect correspondence; or, on the other hand, it has had no lower limit marking perfect absence of correspondence; in either case, we learn little as to the size of the correlation. More often the expression has been such as to have no constant limits at all, but to vary according to the number of cases tested; or it has even depended on special arrangements of the data, that would not apply to correlations between other things, or between the same things under other experimental circumstances; here the expressions of correlation are well nigh useless, as they cannot be compared with one another. Moreover, all these methods have lacked the fundamental requisite, a valuation of the 'probable error.'

Of the more scientifically devised methods, very few meet the requirement urged in the second section of this paper,—that of measuring the correlation between two series of ranks[1]. And these few suffer from the fatal defect, that their coefficients are not in any way comparable with the r of the standard method. This defect is fatal, partly because the numerous valuable results already obtained by the r method are thereby shut out from comparison, and partly because the r values have an intrinsic significance that can ill be dispensed with[2].

§ 4. *Proposed method.*

The last-named defect vitiated among others a method suggested by myself (but unaccompanied by proofs) two years ago[3]. But I believe now to have overcome this difficulty; the respective results given by this and the r methods turn out to be connected by such a simple relation, that translation from one measure into the other is almost as easy as turning a few pounds into their value in shillings.

The method proposed by me seems also to meet every other main requirement of a 'footrule'; above all, the calculation is so extremely short, that it can often be completed under a minute, and sometimes even in one's head. Such a trifling expenditure of labour could often have saved an investigator from completely misapprehending his own results, the labour perhaps of many months.

---

[1] At any rate without an enormous and quite unnecessary increase of the probable error.

[2] These r values admit further of certain corrective formulae, without which, I believe, psychological correlation-research must remain in large measure delusive (cf. *Amer. J. Psych.* 1904, XIII. pp. 88–101.

[3] *Loc. cit.* p. 86.

This short method will first be explained (by an actual example); then its relation to the r method will be demonstrated; and finally the requisite mathematical proofs will be given (in Appendix).

The example is picked from some experiments recently done by Professor Krueger and myself on the correlation between various intellectual faculties. Eleven persons (21—38 years of age, all students of psychology) were individually tested by Krueger as to their power of adding numbers quickly and accurately. Each person, a week after being tested by Krueger in addition, was examined by myself as to his power of discriminating pitch; two tones were sounded successively and he had to decide which was the higher[1]. When all the tests had been completed, Krueger and I showed each other our respective results. These are given below.

Reagent	Rank for addition (Krueger)	Rank for discrimination of pitch (Spearman)	Gains in rank for pitch as compared with addition
A	7	6	1
B	4	4½	
C	10	10	
D	1	3	
E	6	11	
F	9	8	1
G	11	7	4
H	3	1	2
I	2	2	
J	5	4½	½
K	8	9	
Observed sum of the gains (denoted by Σg) = 8½			

Our method of calculating the correlation is as follows. Let the observed sum of gains be denoted by Σg*. Let the sum of gains to be expected on an average by mere chance be denoted by M; this amounts to $\frac{n^2-1}{6}$, where n is the number of cases in each series (for proof, see App.)[1]. Then the correlational coefficient, say

---

[1] It is hoped to publish soon a full account of these experiments.

* Where Σ, as usual, denotes summation. We need only consider the gains, not the losses also, because the sum of the one must necessarily be equal to the sum of the other.

$$R, = 1 - \frac{\Sigma g}{M}\,{}^{*}.$$

In the above case $n = 11$, so that $M = \frac{11^2 - 1}{6} = 20$. Thus the correlation between addition and pitch

$$= R = 1 - \frac{\Sigma g}{M} = 1 - \frac{8{\cdot}5}{20} = \frac{11{\cdot}5}{20} = 0{\cdot}57.$$

The probable error may be taken with sufficient nearness as being

$$= \frac{0{\cdot}43}{\sqrt{n}} \text{ (for proof, see App.).}$$

In the above case, this becomes

$$\frac{0{\cdot}43}{\sqrt{11}} = 0{\cdot}13.$$

To utilize these two values, the coefficient R and its probable error, it must be remembered that in this (and in almost every) probability formula, *any experimental result such as R has no scientific significance —except negatively—unless it be at least twice as great as its probable error;* for otherwise, it is almost as likely as not to be a mere chance coincidence. To be fairly good evidence, the R (or other analogous result) must be over three times greater than its probable error. For a perfectly satisfactory demonstration, the R should be not far short of five times greater[3].

In this way we learn that our correlation between the faculty of adding numbers and that of discriminating pitch is just about large enough to be beyond all reasonable suspicion of mere chance coincidence. Had it been only three times greater than the probable error, *i.e.* 0·39, the danger of mere chance would have been small, but not quite negligible.

---

[1] In practice one usually has to consider several series of the same length. In such case, however much Σg may vary, M remains always the same and only requires to be reckoned once for all.

* Any *minus* result to this equation means that the correlation is inverse. Inverse correlations large enough to be appreciable should be avoided; this is easily done by ranking one of the series in the reverse order (this reversal of ranks usually modifies the result, but only to an extent that is much smaller than the probable error and therefore negligible).

[3] These relations are deduced from the fact, that the larger accidental deviations of a result from its normal value are much rarer than the smaller ones. With values distributed according to the common probability curve, a deviation twice as great as the probable error will occur by mere chance no less than once out of six trials, on an average; a deviation three times greater than the probable error occurs only once out of twenty-three trials; a deviation five times greater, not once out of 1,000 trials.

The probability relations brought to light by the formulae teach several facts of general interest for experimental methodics. One sees that, when a high correlation (say, $R = 0{\cdot}50$) exists between two things, its existence may be satisfactorily proved by—as a minimum—about a dozen cases.

But, on the other hand, it is clearly no good attempting to demonstrate a low correlation (say, $R = 0{\cdot}20$) without first collecting for this purpose at least a hundred cases. And, similarly, about a hundred cases would be required to prove that *no* correlation over 0·20 existed. While to prove with certainty the absence of any correlation over 0·02, one would need ten thousand cases.

One may further note the danger of the common custom of gathering all the data into a small number of groups. Suppose, for instance, that one were determining whether the 'spatial sense' of children alters with their age. A not unusual procedure would be to test the spatial sense of about 100 children, group these, say, into three classes according to age, and then simply see whether the average spatial sense and the average age varied concurrently. But the mere observation of any such concurrent variation would be scientifically worthless, for the odds are only two to one against it occurring just by chance. Now, had we calculated the correlation direct from the individual cases, '*n*' would have been, not 3, but 100, and the reliability of our result would have risen accordingly; we could have determined the existence and precise amount of any correlation, with the small probable error of $\frac{0{\cdot}43}{\sqrt{100}} = 0{\cdot}04$*.

Moreover, the correlation between averages of classes varies enormously with the size of the classes; and as such size differs from one experiment to another, the respective correlations admit of no comparison together. Whereas, if the correlation be calculated from the individual cases, its value is wholly unaffected by the quantity of the cases[1], and all coefficients are strictly comparable.

It should be mentioned that the large probable error given by the above formula for use with R is only necessary when weighing the evidence as to the *existence* of any real correlation at all. When once this existence has been sufficiently proved and when our sole aim is to estimate the *accuracy* of the coefficient, another and much less severe probable error becomes applicable. It depends not only on the number of cases but also on the size of the observed correlation; the rule is that, the larger the correlation, the smaller its probable error. Of this nature is the probable error according to the formula devised by Pearson for the r method. On the other hand, that for the R formula has the great advantage of remaining sufficiently accurate even for correlations derived from no more than half a dozen cases, a fact that has not been demonstrated for the Pearson formula. On the r and R methods being brought into relation with one another (see next section), each becomes able to avail itself to some extent of the special advantages of the other's probable error.

---

* There are, it is true, other formulae, by which one may reckon whether the differences between the respective averages could have arisen by mere chance; but in any case the probable error is much larger than when calculating the correlation between the individual cases.

[1] Except, of course, for merely accidental fluctuations, which will be confined within the limits indicated by the probable errors.

§ 5. *Relation of the short method to the standard method.*

Pearson's formula for the standard method is:

$$r = \frac{\Sigma xy}{n\sigma_1\sigma_2},$$

where x and y are the deviations of any corresponding pair of characteristics from their respective averages, and Σxy denotes the sum of all the values of xy; σ_1 and σ_2 are the 'standard deviations[1]' of x and y respectively; and n is the number of pairs. The practical working out involves, as is well known, very tedious arithmetical operations.

Now, when the same method is applied to a correlation between two series of ranks, the formula admits of a remarkable simplification. We get

$$r = 1 - \frac{\Sigma d^2}{m},$$

where d is the numerical difference between any corresponding pair of ranks, and m is the mean value of Σd^2 by mere chance, being

$$= \frac{n(n^2-1)}{6} {}^*.$$

---

[1] The 'standard deviation' is the square root of the average of the squares of the deviations.

* The identity (in case of ranks), $\frac{\Sigma xy}{n\sigma_1\sigma_2} = 1 - \frac{6\Sigma d^2}{n(n^2-1)}$, can be so easily tested practically, that a mathematical demonstration seems here unnecessary. A few 'ties' of rank will produce a discrepancy, but so slight as to be wholly negligible.

By this simplification the calculations are cut down in a way that should be welcome enough to those whose time is being lavished upon the usual procedure.

In the case of any ranks 'tying,' fractions may be conveniently avoided by doubling each difference before squaring. The formula then becomes

$$r = 1 - \frac{\Sigma\,(2d)^2}{4m}.$$

If we apply this formula to the same illustration as before, we get

Reagent	Rank for addition	Rank for discrimination of pitch	$(2d)^2$
A	7	6	4
B	4	4½	1
C	10	10	0
D	1	3	16
E	6	11	100
F	9	8	4
G	11	7	64
H	8	1	16
I	2	2	0
J	5	4½	1
K	8	9	4
			$\Sigma\,(2d)^2 = 210$

Then $$4m = \frac{4n\,(n^2-1)}{6} = \frac{4 \times 11\,(11^2-1)}{6} = 880.$$

So that $r = 1 - \frac{210}{880} = 0{\cdot}76$ (the next two decimals are 14). If r be worked out by the usual laborious method, it again comes to 0·76 (the next two decimals being 10). Hence the discrepancy caused by the tie amounts only to 0·0004.

In this simplified form, the standard or r method shows itself to be solely distinguished from our short or R method by the fact that the differences of rank *are squared*[1]. The effect of squaring is to give more 'weight' to the extreme differences as compared with the medium ones. This is probably a considerable advantage in most physical measurements. But in other fields of research, and perhaps above all in psychology, these extreme cases are just the ones of most suspicious validity, so that the squaring is here more likely to do harm than good.

---

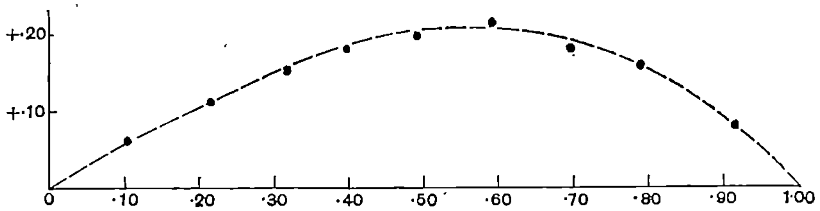
[1] In the R method we needed to consider the sum of the gains only, because this must always be exactly the same as that of the losses. But such equality no longer holds good after squaring; hence here both sorts of difference, losses as well as gains, must be taken into reckoning.

But the discovery of this simple relation between the processes of arriving at R and r respectively is far from telling us at once the relation between the magnitudes of R and r themselves. Turning now to this task, we first of all have to choose whether our proof shall be theoretical or practical. At first sight, a theoretical proof might seem much more complete and scientific. But two important facts have to be taken into consideration. The one is that correlations may conceivably have very *varied forms*; while some may be 'rectilinear' (*i.e.* based on simple proportionality between the two species of values compared), others may possess any kind of 'skewness.' The second fact is that on such variation of the correlation's form, a change must also be expected in the relation between the magnitudes of any two coefficients based—as is the case with R and r—on different powers of the data; the changing relation of such coefficients is in fact the recognized mathematical means of precisely determining the form of a correlation. It is thus clear that any theoretical proof of a constant relation between R and r is quite impossible, except so far as it can be supported by an empirical proof that the class of correlations in question does not vary much in form. But this required empirical proof of a constant form will itself consist, as just mentioned, in an empirical proof of a constant relation between such coefficients as R and r, based on different powers of the data. Thus any attempt at a theoretical proof of a constant relation between R and r is at once thrown back upon an empirical proof of the same relation; whichever route we choose, the validity of our argument always remains coextensive with the validity of the empirical proof. The present paper, therefore, may well content itself with such empirical evidence; this will be the most direct demonstration possible, that our proposed formula connecting R and r actually holds good in all the very various types of psychological correlation from which our observations have been derived.

Incidentally, such evidence will perform a much more important office than the originally intended mere enhancement of the usefulness of the R-method. For until any class of correlations has been shown to possess some constancy of form, such correlations can scarcely be said to be *measurable at all.* This may be most readily seen by assuming the extreme case, that any correlation may be of any conceivable form whatever; then, obviously, any actually observed relations whatever between any two compared series may—for all we can know—represent a perfect correlation. To measure any correlation quantitatively, it is indispensable to make some assumption as to the ideal form, and then to determine how much the observed facts fall short thereof.

The present discussion has, it is hoped, helped to justify the requirement urged in section 2, that psychological correlations should always be based upon values properly comparable with one another (such as are furnished, in general, by comparative ranks). No constancy of correlational form, nor indeed any other kind of regularity, can fairly be expected so long as the calculations are based upon the absolute measurements, whose quantitative relation to the things to be measured are unascertainable—and even, in large measure, arbitrary.

So much for the nature and importance of our task; it is to consist of an empirical proof of some constant relation between the magnitude of R and that of r. First of all we may note that the values of these two symbols already by definition coincide with one another as regards their extreme upper and lower limits, since both coefficients become 1 for perfect correlation and 0 for entire absence of correlation. The problem is to find out what relation connects them for all intermediate values. For this purpose I have worked out both the R and the r of one hundred and eleven correlations that I have actually come across[1]. These have then been gathered into nine groups according to magnitude, so as to obtain at every intermediate tenth an average R and its corresponding r, whereby we can at once inspect the relation between the two throughout the whole range of possible correlation[2]. In the following graph, the base line represents the entire range of R from 0 to 1; the heights of the dots above the base line represent the excesses of r over R actually given by the above-mentioned averages; and the



---

[1] In experiments, school class lists, etc. For calculation of both R and r, any series originally expressed in absolute measurements were converted into series of comparative ranks.

[2] There is evidently a slight error involved in applying the formula to averages instead of to each correlation singly, but this is so slight as to be negligible.

height of the curve above the base line shows how much any r would be larger than its corresponding R, if the following relation were true:

$$r = \sin\left(\frac{\pi}{2}R\right)^{*}.$$

As may be seen, the correspondence between the curve and the dots—that is, between theory and observation—is all that could be desired.

The same result is shown, less vividly but more precisely, in the following table. Here the crucial test is furnished by the right-hand column, whose values ought, by the theory, to remain generally constant from top to bottom, and whose total average should closely approximate to 1.

The correlation R lies between the limits	No. of correlations observed	Average of R	Average of r	$\sin\left(\frac{\pi}{2}R\right)$	$\sin\left(\frac{\pi}{2}R\right)$ divided by r
·06—·15	9	·107	·169	·167	·988
·16—·25	16	·211	·321	·325	1·001
·26—·35	24	·311	·460	·469	1·019
·36—·45	25	·400	·579	·588	1·016
·46—·55	17	·498	·693	·705	1·016
·56—·65	5	·598	·816	·806	·985
·66—·75	9	·691	·862	·884	1·024
·76—·85	4	·787	·945	·945	1·000
·86—·95	2	·915	·990	·991	1·001
Total = 111				Total Average = 1·009 ($\pm$0·003)†	

The point perhaps of greatest practical interest is the size of the discrepancy between r and $\sin\left(\frac{\pi}{2}R\right)$ in the individual correlations; this amounts, on an average, to 0·034. As the average probable error of r

---

* Two years ago I described another relation between R and r, so long as r was between 0·20 and 0·60 (*Amer. J. Psych.* XIII. p. 88, 1904); this I still find to be true enough within such limits, but it turns out to be very inaccurate either above or below them.

† Let the deviations from unity shown by the respective values in the right-hand column be denoted by λ, the number of such values by μ, and the number of correlations entering into each value by p, then, in the usual probability notation, the probable error of the total average

$$= 0{\cdot}6745\sqrt{\frac{[p\lambda\lambda]}{[p](\mu-1)}} = 0{\cdot}6745\sqrt{\frac{0{\cdot}014}{111\,(9-1)}} = 0{\cdot}003.$$

Thus this discrepancy from unity, although so minute, is still three times greater than its probable error, and therefore rather large for mere chance.

is 0·09 (n and r averaging a little over 21 and 0·50 respectively), the discrepancy is not much more than a third of it. Only three times out of all the 111 correlations did the discrepancy actually exceed the probable error (of r), and then barely.

It is particularly important to note that this discrepancy, small as it is, seems to be wholly dependent on the size of n. In the great majority of our correlations, the series were of very nearly equal length, n averaging 20·6; for these the average between r and $\sin\left(\frac{\pi}{2}R\right)$ was 0·035; if we multiply the discrepancy by the square root of n, we get $0{\cdot}035 \times \sqrt{20{\cdot}6} = 0{\cdot}16$. In only nine correlations was the length distinctly different, n here averaging 31·8; for these the average discrepancy between the two formulae sank to 0·029; again multiplying the discrepancy by $\sqrt{n}$, we get $0{\cdot}029 \times \sqrt{31{\cdot}8}$, which once more $= 0{\cdot}16$. Thus the average discrepancy between r and $\sin\left(\frac{\pi}{2}R\right)$ seems to diminish, just like the probable error, in proportion to the increase of $\sqrt{n}$, that is to say, in proportion to the reliability of the evidence[1]. Like the probable error itself, the discrepancy between the two formulae seems to be wholly due to the accidental deviations of the actual correlational coefficients from their true value (that which would be obtained from series of infinite length). No discrepancy remains over to be explained by any real variation of the *form* of the correlation. Hence, this variation of form would seem to be vanishingly small; all these psychological correlations which form the basis of the present investigation, although derived from very varying sources, approximate exceedingly closely to one constant form.

In general, we seem entitled to consider the discrepancies between the two formulae as being too small to be sensible[2]. As far as they do occur, they are due to the mere accidental errors of the two correlational coefficients, and are as much the fault of the standard as of the short method. When we have obtained R—a calculation performed in a minute or so—we can easily enough get r, either by working out the formula, or by using the table given below. In my own experience the r thus derived from R has proved an actually more reliable value than r calculated directly in the usual manner; that is to say, the former has accorded more closely than the latter with the r found (even by direct calculation) in other quite similar experiments. (If, as is usually the case, R does not exceed 0·50, r may simply be taken as $= 1\frac{1}{2} R$, without error of importance for most psychological purposes.)

---

[1] This result has been confirmed by all later observations of the present writer.

[2] Should such discrepancy ever in any exceptional case become really marked, it performs the valuable office of indicating that the correlation in question departs from the common form.

To sum up, the R method seems to proffer a remarkably handy and yet quite accurate enough 'footrule' for measuring correlations. By using it, we obtain a precise quantitative value, which can be compared with that found for any other correlation, under any other circumstances or between any other things, either by the same or by the standard r method; we completely free ourselves from various illusions, which are otherwise almost irresistible; we get a reliable estimate as to the danger of our result being merely an accidental coincidence; and we even learn how to plan out our experiments from the outset in a manner properly adapted to the object in view.

Further, incidentally, some evidence has been brought to show that psychological correlations in general approximate to one constant form. Hereby they prove themselves to be really measurable quantitatively (and, what almost comes to the same thing, to be properly expressible by one single correlational coefficient).

Table to compare together the two correlational coefficients R and r, the latter being taken as $= \sin\left(\frac{\pi}{2} R\right)$.

R	r	R	r	R	r	R	r	R	r
·00	·00	·20	·31	·40	·59	·60	·81	·80	·95
·01	·01	·21	·32	·41	·60	·61	·82	·81	·96
·02	·03	·22	·34	·42	·61	·62	·83	·82	·96
·03	·05	·23	·35	·43	·62	·63	·84	·83	·96
·04	·06	·24	·37	·44	·64	·64	·84	·84	·97
·05	·07	·25	·38	·45	·65	·65	·85	·85	·97
·06	·08	·26	·40	·46	·66	·66	·86	·86	·98
·07	·11	·27	·41	·47	·67	·67	·87	·87	·98
·08	·13	·28	·43	·48	·69	·68	·88	·88	·98
·09	·14	·29	·44	·49	·70	·69	·88	·89	·99
·10	·16	·30	·45	·50	·71	·70	·89	·90	·99
·11	·17	·31	·47	·51	·72	·71	·90	·91	·99
·12	·19	·32	·48	·52	·73	·72	·90	·92	·99
·13	·20	·33	·50	·53	·74	·73	·91	·93	·99
·14	·22	·34	·51	·54	·75	·74	·92	·94	1·00
·15	·23	·35	·52	·55	·76	·75	·93	·95	1·00
·16	·25	·36	·54	·56	·77	·76	·93	·96	1·00
·17	·26	·37	·55	·57	·78	·77	·94	·97	1·00
·18	·28	·38	·56	·58	·79	·78	·94	·98	1·00
·19	·29	·39	·57	·59	·80	·79	·95	·99	1·00

APPENDIX.

I. *Proof that M, the mean value of Σg by pure chance,* $= \dfrac{n^2-1}{6}$.

Let S, $= 2\,\Sigma g$, denote the sum of the differences of rank between *any* two series consisting each of n terms (see p. 95); let $M(S)$ denote the mean value of all the possible combinations of S; then,

$$S = |h_1 - 1| + |h_2 - 2| + \ldots + |h_n - n| = \sum_{\alpha} |h_\alpha - \alpha|$$

and
$$M(S) = M(\sum_{\alpha} |h_\alpha - \alpha|) = \frac{1}{n}\sum_{\alpha\beta} |\beta - \alpha|$$

(since the number of possible combinations between α and $\beta = n^2 = n$ times the number of terms in S)

$$= \frac{1}{n}\sum_{\alpha} \{|1-\alpha| + |2-\alpha| + \ldots + |n-\alpha|\}$$

$$= \frac{1}{n}\sum_{\alpha} \{(\alpha-1) + (\alpha-2) + \ldots + 1 + 0 + 1 + 2 + \ldots + (n-\alpha)\}$$

$$= \frac{1}{n}\sum_{\alpha} \left\{\binom{\alpha}{2} + \binom{n-\alpha+1}{2}\right\}.^{*}$$

$$= \frac{1}{n}\left\{\binom{n+1}{3} + \binom{n+1}{3}\right\}$$

$$= \frac{2}{n}\cdot\frac{(n+1)\,n\,(n-1)}{1\times 2\times 3} = \frac{n^2-1}{3}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (a)$$ [1]

And as all the possible combinations are *a priori* equally likely to occur, this, $\dfrac{n^2-1}{3}$ may be taken as the mean value of S by pure chance, so that

$$M = \frac{n^2-1}{6}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (b)$$

---

* Where $\binom{\alpha}{2}$ denotes, as usual, the binomial coefficient, $\dfrac{\alpha(\alpha-1)}{1.2}$.

[1] This equation was arrived at in a different way by G. Lipps (see *Amer. J. Psych.* 1894, XIII. p. 86).

II. *Proof that the 'probable error' of R approximately* $= \dfrac{0{\cdot}43}{\sqrt{n}}$.

i. Let us commence by determining the mean square deviation of $2\,\Sigma g$ from $M(S)$, that is, of S from $\dfrac{n^2-1}{3}$,

$$M\left(\frac{n^2-1}{3}-S\right)^2 = \left(\frac{n^2-1}{3}\right)^2 - 2M(S)\,.\,\frac{n^2-1}{3} + M(S^2)$$

$$= -\left(\frac{n^2-1}{3}\right)^2 + M(S^2). \quad \dots\dots\dots\dots\dots\dots\dots\dots(c)$$

We require, then, to calculate $M(S^2)$,

$$S^2 = \sum_{\alpha\beta} |h_\alpha - \alpha|\,|h_\beta - \beta|$$

$$= \sum_{\alpha} (h_\alpha - \alpha)^2 + \overset{*}{\sum_{\alpha\beta}} |h_\alpha - \alpha|\,|h_\beta - \beta|$$

where the star over Σ indicates summation only of pairs of values $\alpha \gtrless \beta$ (whereas Σ without a star shows that α and β run independently of one another from 1 to n).

Therefore

$$M(S^2) = \frac{1}{n}\sum_{\alpha\beta}(\beta-\alpha)^2 + \frac{1}{n(n-1)}\overset{*}{\sum}\overset{*}{\sum_{\alpha\beta\gamma\delta}} |\gamma-\alpha|\,|\delta-\beta|. \quad \dots(d)$$

The first component,

$$\frac{1}{n}\sum_{\alpha\beta}(\beta-\alpha)^2 = 2\,\Sigma\alpha^2 - \frac{2}{n}(\Sigma\alpha)^2$$

$$= \frac{n(n+1)(2n+1)}{3} - \frac{n(n+1)^2}{2} = \frac{n(n^2-1)}{6}. \quad \dots\dots\dots(e)$$

As regards the second component of the equation (d), $\overset{*}{\sum\limits_{\alpha\beta}}$ may be transformed into $\sum\limits_{\alpha\beta} - \sum\limits_{\alpha}(\beta = \alpha)$.

Then $\overset{*}{\sum\limits_{\alpha\beta}}\overset{*}{\sum\limits_{\gamma\delta}} |\gamma-\alpha|\,|\delta-\beta| = \sum\limits_{\alpha\beta}\overset{*}{\sum\limits_{\gamma\delta}} |\gamma-\alpha|\,|\delta-\beta| - \sum\limits_{\alpha}\overset{*}{\sum\limits_{\gamma\delta}} |\gamma-\alpha|\,|\delta-\alpha|$

$$= \sum_{\alpha\beta}\sum_{\gamma\delta} |\gamma-\alpha|\,|\delta-\beta| - \sum_{\alpha\beta}\sum_{\gamma} |\gamma-\alpha|\,|\delta-\beta| - \sum_{\alpha}\sum_{\gamma\delta} |\gamma-\alpha|\,|\delta-\alpha|$$

$$+ \sum_{\alpha}\sum_{\gamma} |\gamma-\alpha|$$

$$= \sum_{\alpha\beta\gamma\delta} |\gamma-\alpha|\,|\delta-\beta| - 2\sum_{\alpha\beta\gamma} |\gamma-\alpha|\,|\gamma-\beta| + \sum_{\alpha\beta}(\beta-\alpha)^2$$

$$= \Big\{\sum_{\alpha\gamma}(\gamma-\alpha)\Big\}^2 - 2\sum_{\gamma}\Big\{\sum_{\alpha}|\gamma-\alpha|\Big\}^2 + \sum_{\alpha\beta}(\beta-\alpha)^2.$$

The first term on the right of the above equation is obviously

$$= \{nM(S)\}^2,\ \text{which by } (a) = \frac{n^2(n^2-1)^2}{9}. \quad \dots\dots\dots(f)$$

The third term is by (*e*)

$$= \frac{n^2(n^2-1)}{6}. \quad \ldots\ldots\ldots\ldots\ldots\ldots (g)$$

Lastly, the second term

$$2\sum_{\gamma}\{\sum_{a}|\gamma-\alpha|\}^2 = 2\sum_{\gamma}\left\{\binom{\gamma}{2}+\binom{n+1-\gamma}{2}\right\}^2$$

$$= 2\sum_{\gamma}\left\{\gamma^2-(n+1)\gamma+\frac{n(n+1)}{2}\right\}^2$$

$$= 2\sum_{\gamma}\left\{\gamma^4-2(n+1)\gamma^3+(n+1)(2n+1)\gamma^2-n(n+1)^2\gamma+\frac{n^2(n+1)^2}{4}\right\}$$

$$= \frac{n(n+1)(2n+1)(3n^2+3n-1)}{15}-n^2.(n+1)^3+\frac{n(n+1)^2(2n+1)^2}{3}$$
$$-n^2(n+1)^3+\frac{n^3(n+1)^2}{2}$$

$$= \frac{n(n^2-1)(7n^2-8)}{30}, \text{ on reduction. } \ldots\ldots\ldots (h)$$

Combining equations (*c*), (*d*), (*e*), (*f*), (*g*), and (*h*), we get

$$M\left(\frac{n^2-1}{3}-S\right)^2 = -\frac{(n^2-1)^2}{9}+\frac{n(n^2-1)}{6}$$
$$+\frac{1}{n(n-1)}\left\{\frac{n^2(n^2-1)^2}{9}-\frac{n(n^2-1)(7n^2-8)}{30}+\frac{n^2(n^2-1)}{6}\right\},$$

which reduces to

$$\frac{(n+1)(2n^2+7)}{45}. \quad \ldots\ldots\ldots\ldots\ldots\ldots (i)$$

As the term on the left of the above equation is the mean square deviation of $2\Sigma g$ from $M(S)$, we get the 'standard deviation' of Σg from M

$$= \tfrac{1}{2}\sqrt{\frac{(n+1)(2n^2+7)}{45}}. \quad \ldots\ldots\ldots\ldots\ldots (j)$$

ii. To find thence the *median* deviation of Σg, I was obliged to proceed empirically[1]. Many series of numbers were obtained in such a way, that the distribution of values could be considered that of pure chance. These series were then taken by pairs; for each pair the median deviation of Σg from M was actually observed, and then compared with the standard deviation according to equation (j); the results were as follows:

---

[1] After I had unsuccessfully tried to determine the probable error directly, Prof. Hansdorff kindly suggested, as being a much easier task, the determination of the mean square error. He had taken the trouble to work this out, but then had unfortunately mislaid his calculations; however in his letter to me, he indicated his result and the general course of his proof. Thus, while the credit of this part of the proof is wholly his (as already mentioned by me in the *Amer. J. Psych.*), any faults in the calculations given here lie only with the present writer.

Number of pairs of series	Size of n	Median deviation of Σg actually observed[1]	Standard deviation of Σg by equation (j)	Median ÷ standard deviation
12	11	2·8	4·3	0·66
47	24	9·1	13·1	0·70
16	48	22·4	35·8	0·63
				Average = 0·66 *

It is thus evident that the ratio of the median to the standard deviation (given in right-hand column) may be taken—at any rate with sufficient approximation for our present purpose—as being the same as that for a normal probability curve, *i.e.* 0·6745. The median deviation of Σg becomes then, by equation (j),

$$= 0{\cdot}6745 \times \tfrac{1}{2}\sqrt{\frac{(n+1)(2n^2+7)}{45}} = P, \text{ say.} \qquad \ldots\ldots\ldots(k)$$

iii. We get finally, as the sought probable error of R,

$$1 - \frac{M \pm P}{M}$$

$$= 1 - 1 \pm 0{\cdot}6745 \times \tfrac{1}{2}\sqrt{\frac{(n+1)(2n^2-7)}{45}\cdot\frac{6}{n^2-1}}$$

$$= \pm\, 0{\cdot}6745\sqrt{\frac{2}{5n}}\left(1+\frac{1}{n}\right)^{\frac{1}{2}}\left(1+\frac{7}{2n^2}\right)^{\frac{1}{2}}\left(1-\frac{1}{n^2}\right)^{-1}$$

$$= \pm\,\frac{0{\cdot}4266}{\sqrt{n}}\left(1+\frac{1}{2n} + \text{terms divided by higher powers of } n\right).$$

The terms on the right of the preceding equation form a very rapidly convergent series when n is not exceedingly small, so that the

$$\text{probable error of } R = \pm\,\frac{0{\cdot}43}{\sqrt{n}},$$

with two correct decimals when n is not less than 10, and with sufficient accuracy for practical purposes when n is not less than, say, 6.

---

[1] The plus and minus deviations were very nearly equal.

* This result is not appreciably altered when the values in this (right-hand) column are 'weighted,' according to the amount of observational data upon which they are respectively based.

PROCEEDINGS OF THE PSYCHOLOGICAL SOCIETY.

January 27, 1906. Demonstration of new Apparatus for investigations in Association, by C. S. MYERS.

On certain Cutaneous Perceptions, by CARVETH READ.

Hard-soft, rough-smooth, etc. are not to be treated as sensations, but as perceptions of qualities at an early stage of the perception of body.

Cutaneous perceptions constitute a preparation for the higher complications of the distance-senses.

They are not merely fusions, but complications of pressure and kinæsthesis. Complication with the kinæsthesis is the condition of all perception and of the transmutation of sensations into qualities. The influence of the kinæsthesis upon cognitions that have grown in connexion with it, remains even when it is not directly stimulated.

A comparison of perceptions of different degrees of complication and integration suggests that it is misleading to treat the distinction of perception and judgment as fundamental.

The Relative Effects of Fatigue and Practice produced in the same Individuals by two different kinds of Mental Work, by J. H. WIMMS.

This research has been directed towards the determination of the relative effects of fatigue and practice (*a*) in easy and (*b*) in difficult mental work. For the purpose in view, twelve subjects were carefully selected from the most reliable boys of an upper Form, their ages varying from 14 to 16 years. They were each set to (*a*) the mental addition of two figures, and (*b*) the mental multiplication of three figures, from Kräpelin's '*Rechenhefte*.' The whole research consisted of eighteen experiments which were divided into three series. Each series comprised three addition and three multiplication tests, and in every case the test consisted of two work-periods of ten minutes, separated by a rest-pause of either 20′, 10′, or 0′.

From the data obtained, work-curves have been constructed and an attempt has been made to determine what amount of correlation exists in the two kinds of task, between improvability, retentivity of practice, fatiguability, and absolute amount of work done.

Treating the data *generally*, it has been found that:—

I. The shorter rest-pause (10′) is (unexpectedly) much more generally favourable with the harder task than it is with the easier one.

II. The relative improvability appears much greater with the harder than with the easier task.

III. At the outset fatiguability is greater with the harder work, but apparently as time goes on the more difficult task becomes less fatiguing than the easier (more mechanical) one.

An examination of the *individual* data has shown that:—

I. Those subjects whose absolute amount of work at the easier task was greatest, generally also did the greatest amount at the harder task. Their improvability, however, did not show a similar correspondence.

II. Those who did most work at addition, to some extent showed the highest improvability; but this was by no means the case with the more difficult task.

III. In both kinds of work, high improvability was combined with high retentivity of practice in many individuals.

IV. To some extent with the easier task, but less so with the harder one great fatiguability was exhibited by those whose improvability was lowest.

The results, III and IV, are inconsistent with the general conclusions of Kräpelin and his school.

In addition to the actual work-values obtained, a large number of introspective notes have been collected, which throw considerable light upon individual differences in muscle fatigue, eye strain, liability to distraction, etc.

March 17, 1906. The Fundamental Form of Mental Interaction, by W. McDougall.

July 14, 1906. Demonstration of the following Apparatus:—

Apparatus for the estimation of the Lengths of Lines and for testing various geometrical-optical Illusions.

Apparatus for the investigation of the Temperature Sense.

A New Form of Cattell's Algometer.

New Apparatus for testing Sensibility to Pain.

A New Form of Mr McDougall's Method for studying Mental Fatigue.

A Memory Apparatus, by W. H. R. Rivers.

A series of Tuning Forks, fitted with an Arrangement for producing from them Tones of Constant Intensity.

A New Form of Sound Perimeter, by C. S. Myers.

Observations on Spatial Contrast, by W. G. Smith and S. C. M. Sowton.

The Influence of Interest on the Capacity for Muscular Work, by W. H. R. Rivers.

The Effects of Prolonged Dark Adaptation of the Eye, by W. McDougall.

VOLUME II MAY, 1907 PART 2

ON THE APPARENT HEAVINESS OF COLOURS.

A CONTRIBUTION TO THE AESTHETICS OF COLOUR.

BY E. BULLOUGH.

[*From the Psychological Laboratory of Cambridge.*]

I. *Meaning of apparent heaviness of colours.*
   The 'rule': darker shades are placed below lighter ones.
II. *Material and Method: Observations.*
III. *Analysis of observations.*
   Statistical and introspective methods.
   Dependence of apparent heaviness on luminosity.
   'Positive' and 'negative' weight-classes.
   Other criteria used by subjects: 1. *landscape-associations.*
      2. *single-colour-preferences.*
      3. *optical illusions.*
IV. *Conclusion: Why do dark colours appear as heavier than lighter ones?*
   Objections to visual associations.
   The effect a complex of ideas of weight with visual impressions.
   Weight-impressions of single colours.

1. *The Object.*

THE idea of the following set of experiments, begun in the spring of 1905, concerning the apparent heaviness of colours, was originally suggested to me by Prof. Lipps' contribution to the '*Festschrift*' dedicated to Helmholtz on his 70th birthday, entitled "Die aesthetischen Faktoren der Raumanschauung" (*Beiträge zur Psych. and Phys. d. Sinnesorgane*, 1891), a suggestion which I gratefully acknowledge. Starting with the idea of a seemingly inherent, but really '*eingefühlte*,' energy in straight and curved lines, manifested by their direction, length, curvature, etc. and of a similar expansive tendency, balanced and counteracted by the constraining force of the boundary lines in geometrical figures, Lipps has endeavoured to account on such a basis for a number of geometrical-optical illusions and for the aesthetic effect produced by such spatial combinations. He has since then elaborated this notion into a more complex and wonderfully subtle system, under the paradoxical name of 'Mechanical Aesthetics,' in his *Raumaesthetik* (1897), has brought it into connexion with his '*Einfühlungs-Theorie*,' in his *Grundlegung der Aesthetik* (1905), and has discussed it again, still more recently, in a paper in the first volume of the *Zeits. für Aesthetik* (1906).

The idea occurred to me whether a feature analogous to the energy in linear or spatial arrangements might not be tested experimentally in the case of colours. The difficulty was to find such an energy in colours. It is evident that in the absence of any direction or definitely circumscribed extension (absence, in so far as any boundary lines are not inherent in the colour, but simply imposed upon it *ab extra*), they cannot possess any activity or energy in the sense of that shown by lines or geometrical figures. Yet they do seem to have a characteristic feature, which in some measure might be considered as analogous to the active upward tendency of a vertical line, or the self-centred balance of a circle: namely their *apparent weight*. By the weight of a colour I briefly mean this: supposing a wall 10 ft. high were to be painted in two different shades of one colour, viz. pink and red, in such a way that both should be in equal proportions, the one occupying the space below the other; should the red be placed below, up to 5 ft. from the floor, and the remaining 5 ft. be painted pink, or should the position of the colours be reversed? It is pretty safe to say that the former course would be adopted. The effect of 5 ft. of red on the top of 5 ft. of pink would give to the majority of people an uncomfortable feeling of top-heaviness, instability and lack of balance, which would be absent from the reverse arrangement of the colours. If the red were at any price to be placed above the pink, the only manner in which this could be done satisfactorily, and without producing a special effect, such for instance as lowering the apparent height of the walls, would be, to give, say, 8 ft. of the total height to the pink, and the remaining 2 ft., as a narrow frieze, to the red. Here again the impression of oppressive top-heaviness might be avoided.

The explanation of these facts (if such be the facts) must obviously be looked for in the relation of the two colours, and of the areas occupied by them, to each other, and I submit here that the solution lies in the apparent differences in weight between the two shades. Red seems of greater weight, density, consistency, or in whatever other way this peculiarity might be expressed, than the pink; hence 5 ft. of pink are easily supported by the same amount of red, but 5 ft. of red on top of the same length of pink would seem to lack the necessary foundation to keep it in its place; again, 8 ft. of pink might be of apparently sufficient strength to prevent the 2 ft. of red, so to-speak, from sinking to the bottom. The impression would be an instance of a complex compounded of the visual (colour-) perception and ideas either of weight or of density or solidity, accordingly as heaviness as such, or consistency, massiveness, or impenetrability, etc. were attributed to the colours[1].

The fact that certain, usually darker tints, are preferred below other, generally paler ones, has practically become a decorative canon, though to my knowledge the legitimacy or universal applicability of such a postulate has never been questioned nor have its psychological foundations been investigated. The 'rule' might be formulated thus: "that dark colours should stand below light ones"; but such a wide statement requires a little more specification. In the case of two shades of the same colour (as red and pink) the matter seems pretty clear and obvious. Not only is there very little hesitation as to the arrangement, namely that the darker shade should be placed below the lighter, but the reason for it is evidently referable to the darkness or lower luminosity of the darker shade. The question becomes more complicated in a combination of two different colour-tones: given two colours as red and blue, or pale-green and pink, one finds very much less certainty about the choice of arrangement. On account of the difficulty of determining, by sight, the relative luminosities, which might unambiguously settle the selection, deviations from the rule frequently occur, due to other factors, such as a personal predilection for one or other of the colours, the 'strikingness' of its brilliancy and saturation, warmth or coldness, dulness or energy, associations with natural objects, such as landscapes, in short any of the innumerable and often apparently quite irrational grounds which determine colour-preferences.

---

[1] This 'idea' of weight or solidity may remain purely abstract, or may in some cases actually be realised in concrete images of a muscular or tactile nature, accordingly as individuals stop at the simple 'representative' stage or proceed to revived sensations. 'Idea' is meant in the following paragraphs to cover all stages lying between these extremes.

The problem, consequently, presents two points, capable of being experimentally tested :

(1) How far does the rule that dark shades or colours be placed below light ones, hold good ? Is it applicable to shade-differences only, or also to tone-differences ? and

(2) What is the principle on which this rule rests ?

It might possibly, as has been suggested by several of my subjects, be due to association with natural objects or phenomena ; or it may, as I ventured to submit, be interpreted as the result of a complication of the colour-sensation with ideas of weight or solidity ; that is to say, it might simply be an application of the mechanical law of gravity through the attribution of weight to colour, and thus form a parallel to Lipps' mechanical-aesthetic interpretation of linear and spatial objects. Exceptions or deviations from this 'weight-principle' (if I may be permitted this short expression) would have to be accounted for by other factors.

The weight-principle itself may in the case of shade-differences be traceable to either saturation or luminosity ; and as regards tone-differences, the colour-tone itself might be adduced to afford an explanation. I believe, however, that luminosity is sufficient for the interpretation of the facts, at least apart from cases, very numerous indeed, where factors, other than the weight-principle become operative.

2. *Material and Method.*

For the purpose of testing these two problems a number of plates were made, to exhibit the colours to be investigated. Each plate contained two geometrical figures of identical shape, and each figure was composed of two colours, placed one above the other, the difference being the inverse arrangement of the colours, in such a manner that the colour which occupied the lower half in the one figure occupied the upper one in the other. The figure chosen was a triangle with two equal sides, the base measuring 12 cm., and each of the other two sides 20 cm. Each triangle was divided horizontally by the line where the two colours met, on the average at a distance of 8·02 cm. from the base. The reason which led me to select triangles for this purpose was this : The impression of the apparent weight of colours is mainly relative ; *i.e.* it is primarily an impression of weight-differences ; and in order to obtain the impression in any marked degree, the colours have to be apperceived as belonging to each other, by being part of *one* object (as a wall, for instance). The spatial connexion between them is therefore of paramount importance, and the point was to present them in such a manner, that they could not possibly be dissociated from each other, nor be apperceived separately as belonging to different objects. Again, the idea of height or verticality is essential, in order that the weight of a colour, either supporting or pressing down upon the other colour, should be clearly felt. Now, a triangle seemed to me to combine both these desiderata. While on the one hand presenting verticality in the most emphatic manner, it brought at the same time the colours into the most forcible proximity to each other, by making each section only part of the whole figure; it certainly did so more than a vertical rectangle, divided horizontally, could have done, and thereby prevented the dissociation of the colours from each other, or the apperception of a single part of the figure rather than the whole. I believe the subjects were materially assisted in this object by the triangular shape of the figure. Its general appearance is shown in Fig. 3 below, and the very fact of the lower half by itself appearing incomplete, the idea of a triangle being once present, would naturally lead the observer to complete it, by at once including the whole figure in one apperceptive act. A vertical rectangle, divided horizontally would, on the other hand, easily and naturally have lent itself to a resolution into two squares, each containing one colour, and each being finished and complete in itself; and thereby the intimate connexion between the colours would have been broken. As it was, this difficulty has not in all cases been successfully overcome, even by the use of triangles. Especially in the case of tone-differences, one individual colour was occasionally so absorbingly interesting, particularly to a certain type of subject, that the spatial relationship of the two colours was ignored, and the question resolved itself simply into one of colour-preferences, instead of weight-differences. And here a defect in the use of triangles appeared which I had not anticipated, but which ought to be pointed out, as neutralising in some cases its advantages. The triangles being divided as they were, it was inevitable that the lower colour should occupy a larger area than the upper one, and persons following simply their colour-preferences, usually selected that triangle in which the preferred colour occupied the larger space, where "there was more of it," after the expression of several subjects; and thus a new criterion for choice was introduced, sometimes systematically for all figures, sometimes only in cases where weight-differences were hardly perceptible and did not offer a sufficiently clear principle for selection. At the same time, this defect, inherent in the use of triangles, revealed an interesting fact to which attention will be drawn later on.

The plates were simply made in the following manner: two pieces of coloured paper were cut into four strips of sufficient width, and were pasted crosswise, side by side on a piece of cardboard (cf. Fig. 1). Another piece of cardboard of the same size, on which two triangles were cut out side by side (Fig. 2), was pasted on to the first piece, so that the four strips of paper were seen in the triangular figures, presenting the appearance of Fig. 3. Since the first point was to test the application of the rule, that dark colours should stand below lighter ones, to *shade-* and *tone-*differences, I arranged two series of six plates each, the first containing only shades of the same colours, while the second showed actual colour-differences. The first was called A and the second E, and each triangle on each plate was lettered *a* and *b*, *a* marking the triangle in which to all appearances the darker colour or shade was below, *b* where it was above. The manner of marking the plates and triangles is shown in Fig. 3.

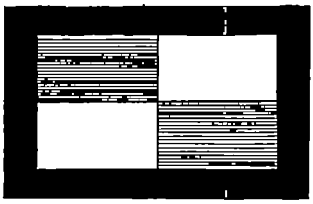


Fig. 1.

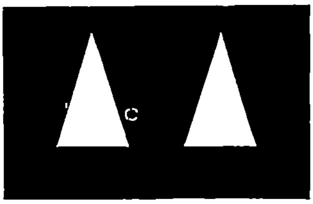


Fig. 2.

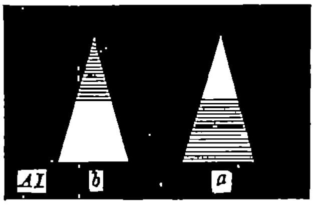


Fig. 3.

Consequently, *a* was the triangle which ought to have been selected, if the weight-principle was applied, though it might, of course, also happen to be preferred for other reasons. The second piece of cardboard (on which the triangles were cut out) was covered with ordinary

French-grey writing paper, so that the colours appeared on a grey, slightly bluish background. In order to vary the conditions a little more, when desirable, I had another piece of cardboard made, with triangles cut out after the pattern of the second, but covered with black paper, so that, by superimposing it on the plates, the colours could be shown on black, thus giving two more series marked respectively B and F; B being the A series (shade-differences) on black, F being the same for E (tone-differences). The advantages of using also a black background only struck me after I had begun the experiments, and consequently a number of subjects were not tested with this variation; in the subjoined table of the results I shall omit these two series, B and F, to save space, but shall give them in the analysis, whenever the results, obtained with them, present special features of interest.

If the weight-principle were to apply to colour-arrangements in which the one colour stood vertically above the other and, so to speak, weighed upon the one below it by the simple action of gravity, it would be interesting to observe the effect of combinations in which the verticality of the colour-sequence is less obtrusive, the question of stability or balance therefore less emphasised and the spatial relationship of the two colours less forcibly impressed upon the subject than is the case in the triangles. I therefore supplemented the main series A and E by four other shorter series, marked respectively C, D, G and H. Each comprised four plates, on each of which two figures containing two shades or colours were shown side by side. C and G presented *squares*, divided *diagonally* by the meeting line of the two colours, C containing only shade-differences and G the tone-differences. D and H contain *circles*, divided *horizontally*, D with differences in shade and H with differences in tone. The manner of marking them was the same as in the case of the triangles, and Figs. 4 and 5 give an idea of their general appearance. It is evident that the relative absence of pronounced

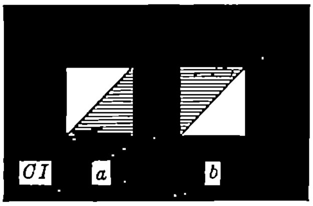


FIG. 4.

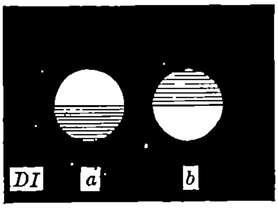


FIG. 5.

vertical extension and the comparative lack of points of orientation (such as are naturally given in the triangles by their base-line) in these four series, might modify or at least weaken the weight-effect of the colours, as compared with the triangles. It would be no longer simply a question of one colour supporting or not being able to support another colour as in the triangles; at best, a square like C I *b* might give one the idea of the darker top-part sliding off the lighter bottom-part, or the circle *b* in D I capsizing and turning itself round into position *a*, to find its balance. The answers, given by some subjects, partly confirm this expectation; often the weight-effect of the colours was even so far weakened that no preference was given to either arrangement; otherwise the heaviness of one colour manifested itself in impressions like those just described.

The usual optical illusions concerning the shape of the figures was, of course, commonly present: thus the *squares* appeared slightly elongated vertically, *i.e.* as vertical rectangles, though I believe, owing to the diagonal line, the illusion was not as pronounced as it would have been, had the square been of a single colour. In any case this illusion can hardly have influenced the results. The *circles* seemed very markedly flattened, *i.e.* appeared as horizontal ellipses, on account of the diameter being visible, and possibly this may in many cases have counteracted the expectation of their capsizing; but as I never heard a remark to this effect from any of the subjects, though plenty regarding their elongated appearance, I am unable to say to what extent this may have been effective, and only mention it as a possible factor of confusion. A somewhat more serious defect, however, I happened to discover in the case of the squares, although not of the nature of an optical illusion. In Figure 4 the two squares are seen side by side, and it will be noticed that the colours on the inside of the square are the same, as indeed was unavoidable; this seems to have suggested to some subjects the idea that the colours ran through underneath the grey intervening space; in other words, that the colour was continuous and that there was "more of it" than of the outside colours. This supposed continuity of the inside colours may have constituted a vitiating factor; if the two figures were not properly separated from each other in the apperception, the impression would have been that of three bands of colour running diagonally across the whole plate and only hidden accidentally in parts by the grey paper (Fig. 6). It is evident that, if regarded in this light, the figures would have lost their individuality and each colour-arrangement would no longer have appeared to be conditioned by its own balance. The defect might have been remedied either by separating the squares from each other by a greater distance or by drawing a black vertical line between them to prevent their being taken as belonging to each other. As such cases however were rare, I thought it best not to introduce a new variation, which would have rendered the latter part of the experiments with this series incomparable with the former part, carried out under the old conditions.

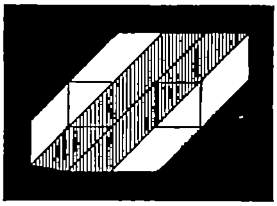


FIG. 6.

Some other optical illusions referable to the colour rather than to the linear elements in the squares and circles will be mentioned later.

All the colours used were shown by coloured papers, pasted on cardboard in the manner described. Some were papers specially pigmented with saturated spectral colours, such as are manufactured for optical experiments, others, particularly dark, pale, broken and mixed tints, I managed to procure in sufficient selection from the bookbinding and wall-paper-trades. In all 30 shades were used as shown in Table I.

A complete list of all the material used and the distribution of the colours is given in Table II.

Altogether 50 subjects were tested with these plates. They varied in age from 8 to about 50; for the greater part they were graduates and undergraduates of the University, between 20 and 40. The youngest subject (eight years old) is number two in the table of results, number seven being the next one in age, a boy of about 12 or 13. The minority included some ladies, and also some men of inferior education; but as will be apparent from the results, the educational status does not affect the point at issue in any remarkable way; whatever divergencies between various individuals do occur, are in my opinion rather to be referred to differences in emotional or generally psychical constitution than to purely intellectual powers or training.

The method of testing was briefly the following: the subject was seated in a chair in an attitude most convenient to himself and the plates were shown to him in sequence at a suitable height, so that his visual plane was at right angles to the plate, to avoid distortions

TABLE I, *giving the colour material and luminosities.*

Colour	Description	Luminosity[1]	No.
I Blue	1 saturated spectral blue ... 2 less saturated, very brilliant 3 paler (dark sky-blue) ... 4 brilliant pale blue ... ... 5 green-blue (turquoise) ... 6 very pale blue	88·66 79·0 78·0 70·0 65·0 31·83	6
II Red	1 saturated crimson ... ... 2 less saturated (rose) ... 3 dark pink ... ... 4 paler pink (salmon) ... 5 very pale pink	79·5 76·5 58·5 48·83 31·66	5
III Green	1 very dark green ... ... 2 dark green ... ... 3 grey-green (pale olive) ... 4 pale green (yellowish) ... 5 pale green (bluish) ... ... 6 bright blue-green ... ... 7 bright yellow-green... ...	86·16 83·5 69·83 60·83 54·5 48·16 32·5	7
IV Brown	1 dark brown (chocolate) ... 2 dark red-brown (terra-cotta) 3 light red-brown ,, 4 pinkish brown	87·0 78·16 54·33 52·0	4
V Yellow	1 saturated yellow ... ... 2 pale yellow ... ... 3 pale green-yellow	29·83 22·16 16·5	3
VI Purple	1 blue-purple ... ... 2 red-purple (magenta) ...	87·16 81·5	2
VII {Green Grey	1 dark ... ... 2 pale	70·0 46·83	2
VIII Orange	1 saturated	59·83	1
	Total of Colours used ...	... ...	30

---

[1] The figures are the percentage of black in the grey compared with each colour. As it was only a question of the relative not absolute luminosity, these values are not corrected by the deduction of the luminosity of the black paper. The figures are the average of six observations on each colour. The luminosities were found by means of Maxwell's rotating discs, after the method described by Rood, 'Colour,' London, 1904. 4th ed. p. 34 (International Scientific Series).

TABLE II, *showing total material and distribution of colours.*

		No. of plates
A, *triangles*, shade-differences	E, *triangles*, tone-differences	
I green-grey 1 + green-grey 2	I blue 1 + orange	12
II blue 6 + blue 3	II purple 1 + green 1	
III green 1 + green 6	III purple 2 + blue 5	
IV yellow 1 + yellow 2	IV green 5 + red 2	
V red 5 + red 2	V red 4 + green 3	
VI brown 2 + brown 8	VI blue 1 + yellow 3	
B, same as A, on black	F, same as E, on black	12
C, *squares*, shade-differences	D, *circles*, shade-differences	
I brown 2 + brown 4	I brown 2 + brown 4	8
II green 2 + green 7	II green 4 + green 5	
III yellow 1 + yellow 2	III blue 5 + blue 6	
IV red 1 + red 4	IV orange + yellow 1[1]	
G, *squares*, tone-differences	H, *circles*, tone-differences	
I blue 2 + red 1	I brown 1 + red 8	8
II green 6 + brown 2	II green 7 + yellow 2	
III yellow 1 + red 2	III brown 4 + green 5	
IV blue 4 + yellow 8	IV red 1 + green 2	
	Total of plates used in the tests	40

of the figure by perspective. They were held at a distance to suit his eyesight, but at the same time far enough away to hide small irregularities in the outlines of the figures or minor blemishes in the colours. As one of the two strips of paper, composing a figure, was slightly overlapping the other to ensure a clean boundary line, care was taken to present the plate at a sufficient distance to prevent the subject from becoming aware which strip was overlapping the other. He was then asked which of the two figures he preferred *as a whole,* time being given to inspect them at leisure, until he was able to make a choice. Cases of hesitation or doubt were noted, and, if desired, a second choice could be made, in which event both were registered. After nearly each exposure and selection I asked the subject to give me the reason of his choice of the figure preferred, or at least the ground for his rejecting the other. Not infrequently subjects pronounced the combinations too hideous to be able to make a choice at all; I then asked to select, if possible, the figure which displeased the less of the two; in fact, all through, I took care to impress upon the subjects the fact that it was less the preferences than the *reasons* for the preferences that I wished to know, and that, in the absence of these, the reasons for rejection, though only negative, might to some extent supply the lack of the former. Often no reason was forthcoming at all in the beginning of the experiments, but with a little practice, some reason or supposed reason was generally discovered. The method was consequently purely introspective, and I was pleasantly surprised to find a definiteness and frankness in most answers which afforded some guarantee of the genuineness of the reasons prompting the choice. I was equally astonished at the amount of introspection in the majority of subjects; there were indeed some with whom it was exceedingly small, but it was always possible to obtain information of some sort, with the exception of no. 27 and no. 32, who were at an absolute loss to account for anything and seemed to live in a most amazing state of ignorance of their own mental life. That the introspective method labours under certain defects and may be vitiated to some extent by self-deception must, of course, be admitted; and it seems to me that more especially in aesthetic experiments on colour-effects, there are particular difficulties attaching to it. The old adage that "there is no accounting for tastes" is perhaps more extensively appealed to concerning colour-impressions than in any other form of aesthetic experiences, and the poorness of effect to which the majority of people seem susceptible, only increases the neglect with which such effects are treated. People hardly ever talk to each other on this point: that one person may like a colour which another detests, is accepted as an ultimate fact, as a preordained arrangement which it would be useless to inquire into. People do not, as a rule, try to explain even to themselves their reasons for their preference or dislike, and a good many subjects were greatly surprised at the idea of being asked to account for them. The majority affirmed that they had never thought about it and confessed that they found some difficulty in giving reasons. Nevertheless, there is no other method for aesthetic experiments; the statistical method of proceeding, as I shall try to show, serving no purpose whatever. It is a case of making a virtue of necessity.

---

[1] This combination ought to be classed under H.

Whenever there was a great dearth of introspection, I tried to elicit some information by questions, proposing a variety of reasons which experience had shown to be operative in other cases. The risk of such 'leading questions' must again be readily conceded, but I think it ought not to be over-estimated. The promptness with which certain suggestions were repudiated, the care and hesitation with which others were weighed before being tentatively accepted and the sudden spontaneity of explanations entirely different, assured me on the whole of the harmlessness of such questions. Above all it testified to the good will, earnestness and sincerity of all concerned in the experiments and I should like to take this opportunity of thanking them again for their readiness to submit to the rather tedious tests, and for the assistance they thereby rendered me. I should also like to express my best thanks to Dr Rivers for the facilities he has given me and for his advice in carrying out the experiments.

I now proceed to the results of the experiments, given in the following Table. Series B and F are omitted, as I mentioned before, and similarly the comments made on the selections by the subjects. These will be mentioned and utilised in the analysis. Blank spaces indicate a few omissions which occurred by oversight; "no preference" is marked by a dash, doubts or difficulties in selection are denoted by a query after the figure chosen.

TABLE III. *Group I.* referring to pp. 127—134.

2		3		4		9		10		11		14		17		18		19	
A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E
Ib	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ib	Ia	Ia	Ia?
IIa	IIa	IIa	IIa	IIa	IIb	IIa	IIa	IIa	IIb	IIa	IIb	IIa	II—	IIa	IIb	IIb	IIb	IIa	IIa
IIIb	IIIb	IIIa	IIIb	IIIa	IIIb	IIIb?	IIIa	IIIa	IIIa	IIIa	IIIa	IIIa	III—	IIIa	IIIa	III—	IIIa	IIIa	IIIa
IVa	IVa	IVb	IVb?	IV—	IVa	IVa	IVa?	IVa?	IVb	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IVb	IVa	IVa?
Va	Vb	Vb	Va	Va	Va	Va	Va	Va	V—	Va	Va	Va	Va	Va	Va	Vb	Va	Va	Va
VI—	VIa	VIb	VIa	VIa	VIb	VIa	VIa	VIa	VIa	VIa	VIa	VIa	VIa	VIa	VIa	VIa	VIb	VIa	VIa
C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D
Ib	Ia	Ia	Ia	I—	Ia	I—	Ia	I	I	Ia	Ib	I—	Ia	Ia	Ia	Ia	Ib	I—	Ia
IIa	II—	IIa	IIba	II—	IIa	II—	IIa	II—	IIa	IIa	IIa	II—	IIa	IIa	IIa	IIa	IIa	II—	IIa
IIIa	III—	III—	IIIb	III—	III—	III—	IIIa	III	III	IIIa	IIIa	IIIa	IIIa	IIIa	IIIa	IIIa	IIIa	IIIa	III—
IV—	IVa	IV—	IVa	IV—	IV—	IV—	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IV—	IVb	IVb?	IVa
G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H
Ia	Ib	Ia	Ib	I—	I—	I—	Ia	I—	I	Ia	Ia	Ib	Ia	Ia	Ia	Ia	Ib	Ia	I—
IIa	IIb	IIb	IIa	II—	II—	IIb?	IIa	II—	II	IIb	IIb	IIa	IIa	IIb	II	IIa	IIb	IIb	IIb
IIIa	IIIb	IIIa	IIIb	III—	III—	III—	IIIb	III	IIIa	IIIa	IIIa	III—	IIIb	IIIa	IIIa	IIIa	IIIb	IIIa	IIIb
IVb	IVa	IVa	IVb	IV—	IVa	IV—	IVa	IV	IVa?	IVa	IVa	IV.a	IV—	IVa	IV	IVa	IVb	IVa	IVa

20		21		23		24		25		26		27		28		29		30	
A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E
Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ib	Ia	Ia	Ia	Ia	Ib	Ib	Ia?	Ia	Ia	I—	Ia	I—
IIa	IIa	II—	II—	II—	IIb	IIa	II—	IIa	II—	IIa	IIb	IIb	IIa	IIa	IIb	IIa	IIb?	IIa	IIa
IIIa?	IIIb?	IIIa	IIIb	IIIa	IIIb	IIIb	IIIa	III—	III—	IIIba	IIIb	IIIa	IIIa	IIIa	IIIa	IIIb	IIIb	IIIa	III—
IVa	IVb	IVa	IVb	IVa	IVa	IVa	IVa?	IVa	IVa	IVa	IVa	IVb	IVb	IVa	IVb	IVa	IV—	IV—	IVb
Va	Va	Va	Va	Va	Va?	Vb	Va	Va	Va	Va	Va	Va	Va	Va	Vb	Va	Va	Va	Va
VIa	VIb	VIb	VIa	VI—	VIa	VIa	VIa	VIa	VIa	VIba	VIa	VIa	VIa	VIa	VIa	VIa?	VI—	VIa	VI—
C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D
I	Ib	I—	I—	Ia	Ia?	Ib	Ib	I—	Ia	Ia	Ia	Ia	Ib	Ia	Ia	I—	I—	Ia	Ia
IIb	IIb	II—	II—	IIa	II—	IIb	II—	II—	IIb	IIb	IIa	IIa	IIb	IIb	IIa	II—	II—	IIa	IIa
IIIb	IIIa	III—	IIIa	III—	III—	III—	IIIb	III—	III—	IIIa	IIIa	IIIa	IIIb	IIIa	IIIa	IIIa	IIIa	IIIb	IIIb
IV	IVa	IV—	IV—	IV—	IVa	IVb	IV—	IV—	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IVa	IV—	IVb	IV—
G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H
Ib	Ib	I—	I—	I—	I—	Ib	Ib	I—	Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ib?	I—	I—	Ia
IIb	IIb	II—	IIa	IIb	II—	II—	IIb	IIb	IIa	IIa	IIb	IIb	IIa	IIb	IIa	IIb	II—	II—	IIa
III	III—	III—	IIIb	IIIa?	III—	IIIa	IIIb	IIIb	IIIa	IIIa	IIIb	IIIa	IIIa	IIIa	IIIa	III—	IIIb	III—	IIIa
IV	IVb	IV—	IVa	IVa?	IVa	IVb	IVa	IVb	IVa	IVa	IVa	IVb	IVa	IVa	IVa	IV—	IV—	IV—	IVa

31		33		35		36		37		39		40		41		42		43	
A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E	A	E
Ia	Ib	Ia	Ia	I—	Ia	Ia	Ib	Ia	Ia	Ia	Ia	Ib	Ia?	Ib	Ia	Ia	Ia	Ia	Ia
IIb	II—	IIa	IIb	IIa	IIb	IIa	IIa	IIa	IIa	IIa	II—	II—	IIb	IIa	IIb	IIa	IIb?	IIa	IIb
IIIb	IIIa	IIIb	III—	IIIa	IIIa	IIIa	IIIb	IIIb	III—	IIIa	III—	III—	IIIa?	IIIa	IIIb	IIIa	IIIb	IIIa	IIIa
IVa	IVa	IVa	IVa	IVa	IVa	IVa	IVb	IVa	IVb	IVa	IVa	IVa	IV—b?	IVa	IVa	IVa	IVa	IVa	IVa
Va?	Vb	Va	Va	Va	Va?	Va	Va	Va	Va?	Va	Va	Va	Va	Va?	Va	Va	Va	Va	Va
VIa	VIb	VIb	VIa	VIa	VIa	VIa	VIb	VIa	VIb	VIa	VIa	VIa	VI—	VIa?	VIa	VIa	VIa	VIa	VIa
C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D	C	D
Ia	Ib	Ib	Ib	I—	Ia	Ia	Ib	I—	Ia	I—	Ia?	Ia?	Ia	I—	I—	Ia?	Ib	Ia	Ia
IIb	II—	IIb	IIa	IIb	IIb	IIb	IIa	II—	IIb	II—	IIa	II—b	II—	II—	IIb	IIa	IIa	IIa	IIa
III—	IIIa	IIIb?	IIIa	IIIa	IIIa	III—	IIIa	IIIa	IIIa	III—	III—	IIIa	IIIb	IIIb	III—	III—	IIIb	IIIb	IIIa
IV—	IV—	IVb	IV—	IVa	IVb	IVb	IVb	IV—	IVa	IV—	IVa	IVa	IVa	IV—	IV—	IVa	IVb	IVa	IVa
G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H	G	H
Ib	Ib	I—	Ib	Ia	Ia	Ib	Ib	Ia	Ib	I—	Ia	Ia	I—	Ia	Ia	Ib	Ia	Ib	Ia
II—	II—	IIa	IIb	IIa	IIa	IIa	IIa	IIa	II—	II—	IIa	IIb	IIa	IIb	II—	IIb	IIb	IIb	IIa
IIIa	IIIb	IIIa	IIIb	IIIa	IIIb	IIIa	III—	IIIb	IIIb	III—	IIIb?	IIIa	IIIb	IIIa?	III—	IIIa	IIIa	IIIa	IIIb
IVa	IVa	IVa	IVb	IVa	IVb	IVb	IVa	IVa	IVa	IV—	IVa	IVa?	IV—	IV—	IV—	IVb	IVa	IVa	IVa

44		45		46		47		48		49		50	
A	E	A	E	A	E	A	E	A	E	A	E	A	E
Iba	Ia	Ia	Ib	Ia	Ib	Ia	Ia	Ia	Ib	Ib	Ia	Ia	Ia
IIa	IIb	IIa	IIb	IIa	IIb	IIb	IIb	IIb	IIb	IIa	IIa	IIa	IIa
IIIa	IIIa	IIIa	IIIb?	IIIb	IIIa	IIIa?	IIIa	IIIb	IIIa	IIIb	IIIa	IIIa	IIIa
IVa?	IVa	IVb	IVa	IVa	IVb	IVb	IVa	IVa	IVb	IVb	IVb	IVa?	IVa?
Va	Va	Va	V—	Va	Vb	Va	Va	Vb	Vb	Va	Va	Va	Va
VIa	VIa	VI—	VIa	VIa	VIb	VIa	VIb	VIb	VIb	VIa	VIb	VIa	VIa
C	D	C	D	C	D	C	D	C	D	C	D	C	D
Ia	Ia	Ia	Ia	Ia	Ia	Ia	Ib	Ia?	I—	Ib	Ib	Ia	Ia
IIa	IIa	IIa	IIa	IIa	IIa	IIa	IIb	IIb	IIb	IIb	IIb	IIa	IIa
IIIa	IIIa	III—	IIIa	IIIa	IIIa	IIIa?	IIIb	III—	IIIa	IIIb?	IIIb	IIIa?	IIIa
IVa	IVa	IVa	IVa	IVa	IVb	IVa	IVa	IVa	IVb	IVb	IVb	IVa	IVa
G	H	G	H	G	H	G	H	G	H	G	H	G	H
Ia	Ia	Ia	Ia	Ib	Ib?	Ia	Ib	Ia?	Ib	Ia	Ib	Ia	Ia
IIb	IIa	IIb	IIa	IIb	IIb	IIb	IIb	IIb	IIb	IIb	IIb	IIb?	IIa
IIIa	IIIb	III—	IIIa	IIIa	IIIb?	IIIb	IIIa	IIIa	IIIb	III—	IIIb	IIIa	IIIa
IVa	IVa	IVb?	IVa	IVa	IVa	IVa	IVa	IVa	IVb	IV—	IVa	IVa	IVa

TABLE III.—*continued.*

Group II. 1 (pp. 134—136).

5		13		38	
A	E	A	E	A	E
Ia	Ia	Ia	Ia	I—	Ia
IIb	IIb	IIa	IIb	II—	IIa
IIIa	IIIa	IIIa	IIIa	IIIa	III—
IVb	IVa	IVa	IVa?	IV—	IVa
Va	Vb	Va	Va	Va	Vb
VIa	VIa	VIa	VIa	VIa	VIa
C	D	C	D	C	D
Ia	Ia	I	I	I—	Ia?
IIa	IIa	IIa	IIa?	II—	IIa
IIIa	IIIa	III	III	III—	III—
IVa	IVa	IV—	IVa	IVa	IVa
G	H	G	H	G	H
Ia	Ia	Ia	I	I—	Ia
IIb	IIa	IIb	II	II—	IIa
IIIa	IIIb	III	IIIb	III—	III—
IVa	IVb?	IV	IVa?	IVa	IVa

Group II. 2 (pp. 136—139).

6		12		34	
A	E	A	E	A	E
Ib	Ib	Ia	Ia	Ia	Ib
IIb	IIa	IIa	IIb	IIb	IIb
IIIb	IIIb	IIIb	IIIb	IIIa	IIIa
IVb	IVb	IVa	IVb	IVa	IVa
Vb	Vb	Va	Va	Vb	Vb
VIb	VIb	VIa	VIa	VIa	VIb
C	D	C	D	C	D
I—	I—	Ia	Ia	Ib	Ib
II—	IIa	IIb	IIa	IIb	IIb
III	III	IIIa	IIIa	IIIb	IIIb
IVa?	IVa	IVa	IVa	IVb	IVb
G	H	G	H	G	H
Ia	Ib	Ia	Ia	Ia	Ib
IIb	IIb	II—	IIa	IIb	IIb
IIIb	III—	III—	IIIb	IIIb	IIIb
IVb	IVa	IVa	IVa?	IVb	IVb

Group II. 3 (pp. 139—143).

22	
A	E
I—	Ia
IIa	II—
IIIa	III—
IVa	IV—
V—	Vb
VIa	VIa
C	D
I—	I—
II—	II—
III—	III—
IV—	IVa
G	H
I—	I—
II—	II—
III—	III—
IV—	IV—

Group III. 1 (pp. 143—145).

1	
A	E
Ia	Ia
IIa	II—
IIIa	IIIa
IVa	IVa?
Va	Va
VIa	VIa
C	D
Ia	Ia
IIb	IIa
IIIa	IIIa
IVb	IVa
G	H
Ia	Ia
IIb	IIa
IIIa	III—
IVa	IVa

Group III. 2 (pp. 145—6).

7		16	
A	E	A	E
Ib	Ib	Ia	Ia
IIb	IIa	IIa	IIb
IIIa	IIIa	IIIb	IIIa
IVa	IVa	IVa	IVa
Vb	Va	Va	Va
VIa	VIb	VIb	VIa
C	D	C	D
Ib	Ib	Ia	Ia
IIa	IIb	IIb	IIb
IIIa	IIIa	IIIb	IIIa?
IVb	IVa	IVb	IVb
G	H	G	H
Ia	Ib	Ia	Ib
IIa	IIa	IIb	IIa
IIIa	IIIb	III—	IIIa
IVb	IVa	IVa	IVb

Group III. 3 (pp. 146—148).

15	
A	E
Ia	Ia
IIa	IIa
IIIb	IIIa
IVa?	IVb
V—	V—
VIa?	VIa
C	D
Ia	I—
IIb	IIa
IIIa?	III—
IV—	IVa
G	H
Ia	Ib
II—	II—
III—	IIIa
IV—	IVa

Two cases not referred to in the text.

8		32	
A	E	A	E
Ib	Ib	Ib	Ia
IIb	IIb	IIa	IIb
IIIb	IIIb	IIIb	IIIa
IVa	IVb	IVb	IVb
Va	Va	Va	Vb
VIa	VIb	VIb	VIb?
C	D	C	D
I	I	Ib	Ib
IIa	IIa	IIa	IIb
III	III	IIIa	IIIa
IVa	IVa	IVb	IVa
G	H	G	H
I—	I	Ib	Ia
IIb	II	IIa	IIa
III	IIIb	IIIa	IIIa
IV	IVa	IVa	IVa

Note: Records referred to on pp. 134—148 and not given under their respective groups, will be found under Group I. or other preceding Groups.

3. *The Analysis.*

I. What is the meaning of the results as given in Table III and how are they to be interpreted?

In Table IV below, compiled from Tables I and II, will be found the luminosities of the colours, forming the combinations, and the letters of the figures, which ought to be preferred on the weight-principle, assuming the weight-principle to be based on the luminosities; and the most obvious course would seem to tabulate statistically the results of Table III and deduce by calculation the preponderance of preferences of the *a*'s, to prove both the existence and the basis of the weight-principle. But a moment's reflection will show the deceptiveness of such a statistical method in this case; and since this is a matter which affects experimental work in Aesthetics generally, I feel bound to substantiate my previous remark, that here statistics serve very little, if any, purpose. Its use in aesthetic experiments I believe even to be to a large extent responsible for the discredit into which the latter have fallen, both in the eyes of laymen and men of science.

The application of statistics to aesthetic experiments dates from Fechner, who, in his investigation into Zeising's principle of the 'golden section,' for instance, has inaugurated their use. He tested a number of subjects with a series of rectangles, one of which was constructed on the golden section-ratio, registered the preferences and deduced statistically his results. His object was to ascertain which form of rectangle was the most universally pleasing, hoping that by a certain consensus of opinion the one, embodying the golden section, would receive the greatest number of votes, and that thereby the applicability of the principle would be proved. He certainly did obtain results which on the whole confirmed his expectations, the majority of choices falling on or near that particular figure. But did he prove thereby the existence of such a principle as the golden section? Would it have been proved, even if all the preferences had been given to the one figure which represented the golden section-ratio? I think, he could claim to have shown nothing but a strong presumption in favour of it, but by no means to have given a proof of its existence. His tacit assumption that, if the rectangle constructed on the golden section-ratio were usually selected, it must be selected for this reason, is after all simply an assumption, incapable of proof by the statistical analysis of his results. What his tests show, and what alone they could show, was that a certain rectangle, the sides of which bore approximately the ratio of 3 : 5, was more generally acceptable than those which differed widely from it. The main result was the statement of a fact which in itself contained no data for its explanation, and the fallacy was the assumption that there must necessarily be only *one* explanation, and that the golden section must needs be the principle accounting for all the preferences, given to the one rectangle constructed on its ratio. Invaluable as the statistic method undoubtedly is, its use is restricted to certain conditions; the mere numerical accumulation of facts can render no proof of the existence of any underlying principle, unless it can be demonstrated or at least reasonably be assumed that all the facts are due to the working of one and the same principle. Yet, in the present state of our knowledge, there is nothing to indicate that in aesthetic preferences one single reason is everywhere active; on the contrary, everything goes to show that a variety of reasons prompts aesthetic selections. The uniform reduction of all aesthetic phenomena to one single principle would mean, at least at present, an indefensible narrowing and impoverishment of Aesthetics as a science, and would make it impossible for it to do justice both to the various forms of aesthetic objects and the individual differences in the experiences. Thus even granting, in the case of Fechner, that the golden section had been operative in some of his subjects, there is nothing to show that other reasons might not have prompted others to the same choice, and by simply disregarding the subjective motives of each individual, he necessarily vitiated his results, by swelling the number of preferences given to the golden section-division by others which were not by any means due to the same cause. The systematic neglect of the individual factor, due to the distrust of introspective evidence as unscientific because it is subjective, and the unjustified assumption that one principle must account for everything, seems to me mainly responsible for the disrepute into which experimental work in Aesthetics has—and it must be admitted, fairly deservedly—fallen. There may be other objections and difficulties such as have been pointed out by Volkelt[1] and Lange[2] for instance; but this, it seems to me, is the real reason for the half incredulous, half contemptuous smile with which the very idea of 'aesthetic' experiments is generally received.

Experimental work in Aesthetics has no justification, if its aim does not go beyond the discovery of generally pleasing objects. Yet strange as it may seem, this has been and is still to a large extent the main object of tests and experiments with simple geometrical figures, rhythms, colours and colour combinations. Aesthetic experiments are not an end but only a means, a means to the comprehension of more complex and intricate aesthetic experiences, by providing relatively simple conditions which may facilitate the understanding, explanation and tracing of the mental states involved in the appreciation of aesthetic objects. The only necessary assumption is the similarity between aesthetic states of a more complicated and those of a simpler type. That there may be besides differences of a qualitative nature, due to the artificial and forced conditions of the experiment, must naturally be admitted and cannot simply be set aside. Still the value of experimental tests will be conceded, if they be regarded as such a means; but in order to attain its object, experimental aesthetics (to quote from an article by Segal[1]) "must be, not statistical, but psychological."

---

[1] (I.) = F. Volkelt, *System der Aesthetik*, München, 1905, Bd. I. S. 36.

[2] K. Lange, "Über die Methode der Kunstphilosophie," *Zeits. für Psychol. u. Physiol. d. Sinnesorgane*, Bd. XXXVI. 1904, S. 395 ff.

It is easy to illustrate from the Table of results the insufficiency or rather fallacy of a statistical tabulation of the objective preferences. Referring to Table IV below, it is evident that if the weight-principle is operative, *a* in A I should be chosen, and taking five cases, it will be seen that nos. 17, 21, 31, and 47 actually do prefer *a* to *b*, while for instance no. 35 records his inability to make a choice. Objectively the four former cases are equivalent to each other and would, if they were the only observed cases, give 100 °/₀ of preferences for the weight-principle; as a matter of fact, there is only one instance (or 25 °/₀) of choice based on it, namely no. 17; no. 21 prefers *a* on account of a landscape-association (cf. below), and no. 47 because he prefers the darker shade of the greenish-grey which in *a* covers a larger area than in *b*. No. 31 belongs to a small, but interesting class, which for want of a better name, I should call 'the negative weight-class'; subjects belonging to this type select as a rule the *b*'s, on account of their greater lightness and more marked triangularity (cf. below) and no. 31 was led to select *a* in this case, because the luminosities of the two shades used in A I, differ only slightly, and *a* consequently does not show the massiveness or squattiness which he otherwise objects to in the *a*'s. No. 35 again, who on the whole is a fairly consistent adherent of the weight-principle, was, for the same reason as no. 31, unable to make up his mind which of the two figures to prefer and therefore abstained from choice. In these four or five cases some of the leading criteria which actuated the selections throughout the tests are represented; and the importance of taking note of the subjective reasons for choice, and the impossibility of obtaining trustworthy results by a statistical classification could, I think, be hardly more strikingly demonstrated.

---

[1] F. Segal, "Über die Wohlgefälligkeit einfacher räumlicher Formen," *Archiv f. gesamte Psychol.* Bd. VII. S. 86.

The first question to be considered was the following: "does the rule that dark tints should be placed below lighter ones, apply only to shade-differences, or both to shade and colour-differences?"

To obtain an answer from the results in Table III it is necessary first of all to eliminate those cases which professedly accept selective criteria other than those inherent in the colour-arrangements themselves for the solution of the problem which colour or shade should stand below the other in the figures. This is not, as it might at first sight seem, arguing in a circle; if, it might be objected, those cases which do not conform to the rule are eliminated, it would not be surprising to find that the remainder do conform to it. But we are not eliminating cases which do not conform to the rule, *because* they do not conform to it, but because the rule, whether followed or not, does not exist for them. We are not concerned with the objective preferences in themselves, but with the reasons on which they are based; and from this point of view, it is evident that cases which are influenced not by the purely spatial relationship of the two colours and their mutual inter-dependence, but by other motives, can have no claim to sway the balance either for or against the above rule, which applies precisely to that spatial relationship. Such cases are, for instance, those of preferences for individual colours; they break the connexion between the two colours contained in one figure, by emphasizing one to the detriment of the other. Theoretically there ought then to be no preference for one figure rather than for the other, since the same colour is found in both; and if, as we shall find, there are nevertheless very strong predilections among individuals of this type, the selection is due to the spatial form of the figure and not exclusively to the colour-arrangement as such. Again, landscape-associations, which are fairly numerous, determine preferences which must here be excluded, as being the result of a purely accidentally operative factor; they too are traceable directly to the linear element of the figures, cooperating with the colour-combination, but independent of the position of the colours relatively to each other, apart from their being placed within such and such a geometrical space. If the colours had been presented on a wall instead of enclosed in a triangle or circle, the landscape-association would probably never have arisen. These are only two classes which can obviously have no voice in the determination of this first point; but there are other factors of a similar nature, and the sometimes very chaotic appearance of the records in Table III is due partly to the constant use of one, and partly to the miscellaneous application of several such principles.

After the elimination of such cases, the remainder are those which make selections on the basis of a mutual influence of the colours on each other and of their spatial relationships to each other. That they indeed adopt the weight-principle in some form or other, is at this juncture simply a coincidence with which we are not at present concerned. It is only interesting as offering a guarantee that the effect of the spatial connexion between the two colours was considered in the selections.

Even a cursory survey of the records of this class in series A and E shows, I think, two points with sufficient clearness:

(1) That the rule, almost without exception in the shade-differences, applies equally to the tone-differences; but that (2) the tone-differences introduce into the selections an element of uncertainty and hesitation, as shown by the occasionally very erratic preferences in series E.

The most convincing cases are those of nos. 11, 14, 17, 26, 41, 42, 43, 44, and 50, for instance. Considered as a class, their characteristic is the remarkable similarity shown in their uniform preferences for the *a*'s in A and also in E, with occasional differences of opinion concerning E II, and E III. Since none of the subjects betrayed any signs of employing various criteria, but consciously adopted one single principle of selection in their preferences, there can be little doubt that the same principle that was active in series A was also applied to series E, both to the shade- and colour-differences. And there can be equally little doubt about the objective features in the figures which led them in their selections. I subjoin here for reference Table IV, which shows the combinations of luminosities in the figures and the selections which ought to be made, if the luminosities are taken as the determinating factor. That the preferences of this class are really based upon the lightness and darkness of the colours, *i.e.* upon their relative luminosities, is, I think, sufficiently evident from the concord of these cases with Table IV, and would hardly require any special proof, even if such proof were not furnished by their introspective evidence. Though the conformity with the postulated selections of Table IV is sometimes perfect in both series, a certain amount of indecision and discrepancy of opinion is neverthless evident in series E, in spite of the complete regularity in series A. Cases representative of this point are for instance, nos. 10, 29, and 36; and I believe, by way of explanation, that the difficulty of comparing the luminosities of two different tones, in comparison with the facility of doing so in the case of shade-differences, is beyond dispute responsible for a good deal of the uncertainty, shown either by inability to choose as in no. 29, or by divergencies of opinion between these subjects in their actual selections.

TABLE IV, *showing combinations of luminosities and selections postulated thereby.*

A			E		
I	70·0 +46·83	*a*	I	88·66+59·88	*a*
II	78·0 +81·83	*a*	II	87·16+86·16	?
III	86·16+48·16	*a*	III	81·5 +65·0	*a*
IV	29·83+22·16	*a*?	IV	76·5 +54·5	*a*
V	76·5 +81·66	*a*	V	69·33+48·88	*a*
VI	78·16+54·38	*a*	VI	88·66+16·5	*a*
C			D		
I	73·10+52·0	*a*	I	78·16+52·0	*a*
II	83·5 +54·5	*a*	II	60·88+54·5	*a*
III	29·83+22·16	*a*?	III	65·0 +31·88	*a*
IV	79·5 +48·88	*a*	IV	59·88+29·83	*a*
G			H		
I	79·5 +79·0	?	I	87·0+58·5	*a*
II	48·16+73·16	*b*	II	82·5+22·16	*a*?
III	76·5 +29·83	*a*	III	54·5+52·0	?
IV	70·0 +16·5	*a*	IV	79·5+83·5	*a*?

(I should mention here, that possibly in the tone-differences the colour-tone itself was not quite without influence on the choice, quite apart from the luminosities; but only occasionally and under certain circumstances. I noticed sometimes that, when there was a great difficulty in deciding between the luminosities of two colours, and the selection was not quite unambiguously settled by reference to it, the subject in his uncertainty clutched, so to speak, at any other criterion that might happen to present itself rather than leave the question undecided. This occurred notably in E II, which in fact was specially devised as test-plate for this emergency. It is not improbable, though I was unable to obtain definite information on this point, that a certain 'strikingness' or other peculiarity of one of the two colours might have helped to solve the doubt; and I confess that I cannot otherwise account for the discrepancies of selections in E II; one would have expected no selection at all, as indeed often happened, if the luminosities alone were considered (cf. Table IV), while, as a matter of fact, the majority of subjects chose either *a* or *b*, even subjects who otherwise were uniformally and exclusively guided by the relative luminosities. But instances of this colour-influence are exceptional and the admission of its occasional effect does not, therefore, invalidate the general assertion that it is the luminosity which forms the principal factor in the preferences, in as much as the colour-tone only acted as a supplementary criterion, if the usual one failed.)

The cases I have quoted so far, as showing the application of the rule to shade- and colour-differences are only the purest and most representative; but the same fact is amply attested by others which are less unadulterated and show the sporadic influence of the working of various principles. The number of such 'adulterated' cases is, as might be expected, very large, but they contribute, notwithstanding their irregular appearance, to the cumulative evidence for this point. Cases in question are: nos. 3, 4?, 9, 18, 19, 20, 23?, 24, 25, 27, 28, 29, 30, 33, 35, 37, 39, 40, 45, 46, 48, and 49?.

As illustration may serve the record of no. 45: Series A I—III and V conform to the rule and similarly E IV and VI. In A IV, *b* is preferred because "it looked less heavy and massive than *a*" (there was often some difficulty with this plate), and in A VI, *a* and *b* are both unpleasant and therefore indifferent. In E I there is a strong objection to the glaring blue and *b* is chosen as containing less of it (in the apex) and as regards E V the result is the same as in A VI, only for the opposite reason; both *a* and *b* are pleasant and the outcome is that no selection is made. Instances therefore, which do not conform to the rule are explicable on special grounds which have nothing to do with the mutual influence of the colours on each other, and consequently can not militate against the rule.

Such irregularity is, as a matter of fact, the usual result, absolutely uniform cases as nos. 17 and 50 being decidedly exceptions; but after a careful analysis of cases seemingly quite chaotic, the fact still remains that the rule applies and even predominates, in spite of frequent divergences from it, in series E, the tone-differences, as well as in A, provided always that the luminosity-differences are sufficiently marked to afford a criterion; otherwise hesitation, uncertainty or even indifference is the result.

II. The recognition of the important part which luminosity plays as the leading factor in determining the selections, brings us face to face with a second question: What is the reason of the 'rule'? The answer, that the *luminosities* form this basis, only begs the question, since that much is already implied in the rule itself. The problem is to link the facts up with some principle of wider application, such for instance as the law of gravity, and the above question really amounts to this: "Is the rule really grounded, as I have suggested, on some weight-principle, *i.e.* on the attribution of heaviness to colours of *less*, and of comparative lightness to colours of *greater*, luminosity? Or is it due to some normative idea, based upon associations with, or revived memories, of, natural objects or phenomena?"

In confirmation of the former view I have to offer evidence of three kinds: the first two as introspective testimony of the subjects, the third as a presumption in favour of it, derived from the objective records of Table III.

1. The testimony of many subjects pointed directly to some kind of weight or heaviness ascribed to the darker colours. It generally took the *negative* form that the *b*'s in A and E, especially in A, looked "top-heavy," and were therefore rejected. Subjects who volunteered this statement, when asked for the reasons of their selections, were very numerous: nos. 5, 10, 13, 17, 23, 35, 36, 37, 41, 42, 43, 44, 45, and 50, all expressed the same impression in practically the same words, namely that *b* was top-heavy. A few went so far as to account *positively* for their preferences, as for instances, that in *a* the darker colour looked more "substantial" (no. 13), that it gave better "support" (nos. 26 and 35), that it appeared more "solid" (no. 43) or imparted to the whole figure a better "balance" (no. 48).

There were some subjects who were unable to offer an explanation spontaneously, though they quite obviously belonged to the same class, both by reason of the regularity of their selections and the absence of other acknowledged principles. When the question, however, was put to them afterwards whether the *b*'s of certain plates did not perhaps look top-heavy or unbalanced, they promptly admitted this impression. It would, of course, be rash to place much stress on evidence of this kind, but as no other reason was given and the results accorded perfectly with this criterion, it might be considered fairly safe to assume that the suggestion accorded with the subjective impression. Instances of this kind are furnished by nos. 20, 27, and 29. I believe that generally the acceptance of the suggestion was quite *bonâ fide.* No. 49 flatly refused to adopt it, by saying that he did not have the impression of top-heaviness, but simply preferred the darker colour below. It was useless to press for a reason nor could any other acceptable motive be discovered. Other persons who were unable to give any account of themselves, were rare on the whole; nos. 3 and 33 confined themselves, as no. 49, to baldly stating that they liked to see the darker colour below, but nothing more, while others, as nos. 24 and 25, though neither giving nor accepting any explanation, were evidently following the usual rule.

Very few individuals seemed to approach the whole question in any prejudiced spirit or with any preconceived notions as to the position which the darker colour *ought* to occupy, the majority confessing that they had never even given a thought to the matter. Nos. 9, 14, and 39 admitted that they were to some extent guided by the normative idea that the darker colour *should* stand below the lighter; but that the impression was in itself sufficiently pronounced to yield regular preferences, is shown by no. 10, who was perfectly aware of the rule and of the whole purport of the tests, yet nevertheless expressed his astonishment that his judgments were as definite and his impression of the weight-differences as marked as they actually were.

Of specially noteworthy features in this connexion only three are to be mentioned: (1) No. 38 stated that he was conscious of top-heaviness only in the case of the squares and circles, while his selections in A and E, though objectively conforming to the rule, were accounted for on other grounds. This is a distinctly unusual result. (2) No. 40 gave an explanation which is equally exceptional: namely that the darker colour "stood out" more than the lighter one and that therefore the latter seemed "more natural" above; it would therefore seem as if some perspective illusion were at work in some of his preferences, though not quite of the kind which we shall meet later. His record is, by the way, very intricate and rather irregular. (3) An interesting point is represented by nos. 11 and 30. No. 11 is objectively quite regular, and afterwards admitted the idea of heaviness; but his first explanation (which in his case may have been the true one, though perhaps only a periphrase of the weight-idea) was "that some colours were more striking than others and that the less striking rested naturally upon them." This "strikingness" was still more marked in no. 30, who indignantly repudiated the idea of weight in colour, and would have it that the apparent heaviness (which he admitted) was due to the more forcible manner in which some colours struck him.

2. If this may be called the '*direct*' introspective proof of the attribution of weight to colour, '*indirect*' introspective evidence is given by a small group of cases which seem to follow what I ventured to describe as the '*negative*' *weight-principle*. We find the representative instance of the working of this criterion in no. 6. Being one of the first cases of this type that came to my notice, I was not a little disconcerted, until I discovered its leading characteristic. It will be seen by reference to Table III that no. 6 systematically prefers the opposite to that which the positive weight-class as a rule selects: in series A the *b*'s are chosen throughout, and similarly in E, with the exception of E II *a*, where others usually prefer *b*. He informed me that his reason for his preferences was, that the *b*'s in his opinion better expressed the "triangularity" of the figures; in other words, by placing the darker colour in the upper half of the triangle, the apex stood out with greater force and distinctness from the grey background, forming a little triangle by itself and thereby emphasizing the vertical tendency, the rising energy, in fact the "triangularity" of the whole figure; the *a*'s, on the other hand, were heavy, massive, clumsy and lacked the peculiar spring and liveliness that a triangle ought to possess. Representatives of this class are very rare, at least of such consistency as no. 6. An instance, however, approaching to his case and showing a feature of special interest, is no. 23. In Table III, where only series A and E are given, he seems (as indeed he does) to belong to the positive weight-class; but when tested with A and E on black, *i.e.* with series B and F, he fairly regularly adopts the negative principle. His record, to be compared with Table III, is: B I—, II—, III *b*, IV *b*, V *b*, VI *b*; F I *b*, II *a*, III *b*, IV *a*, V *a*, VI *b*; and in his comments he says "that in A and E, when he preferred *a*, *b* looked 'top-heavy'; but that in B and F, *a* looked frequently too 'solid' and *b* was consequently preferred." I am inclined to think that this has happened sporadically also in other cases which on the whole conform to the positive principle, but has no doubt often been overlooked in introspection. Instances of which I have become aware as actually due to it are no. 30 (B VI *b*), no. 31 (E VI *b*), no. 33 (A VI *b*), and no. 45 (A IV *b*), all of which agree in stating that *b* looks less heavy than *a*. No. 48, whose results look rather erratic, is in reality following both principles, weighing them

against each other and selecting after much deliberation. He explained to me that the *b*'s often looked "better finished," by which, I think, he meant much the same thing as no. 6 by his "triangularity"; and on E I *b* he remarked, besides commenting on the "better finish" of that figure, that the dark blue top seemed to "weigh down upon the orange and keep it in its place." The impression of weight that he experienced with regard to the dark blue could hardly be more directly expressed, and this selection gives an instance of the attempt to reconcile in one figure the joint action of the positive and negative principles.

At the same time there can be little doubt that in cases of this negative weight-principle, a new factor has come into operation which complicates the simple action of the weight-principle. It is in itself suspicious, that with no. 23 the positive principle, active in series A and E, should have been abandoned for the negative principle in B and F; and especially no. 48 strongly suggested the idea that his "better finish" was partly due to the contrast between the colour in the apex of the triangles and the background. I did not have the impression that the selections of this whole class were by any means due entirely to contrast, but really to a feeling of greater lightness in some, and of an excessive massiveness of other, triangles; and the ground for this belief is principally the introspective testimony of the subjects. Thus I think, for instance, that no. 6 was relatively more free from contrast-effects than no. 48. Still the action of contrast was undeniably present and often consciously noticed by some subjects; and it was for the special purpose of testing the fluctuations, produced by the varying conditions of the background, that series B and F were devised. The black, instead of the grey background, conditioning a light-contrast, might reasonably be expected to alter the luminosities of the colours sufficiently to modify also the preferences. The principal result of substituting black for grey was that the luminosity of the light colours was apparently raised, and that the dark colours lost in perspicuousness. This latter effect was sometimes commented on, by saying that "the dark colours seemed to fuse with the background." These modifications of the luminosities might in their turn be expected to bring about an alteration in the whole balance of the figures by, as it were, changing, however slightly, the counteracting tensions of the two colours, and no. 23 proves that this actually occurred, by following in A and E the positive, and in B and F the negative principle. No. 6 shows similar changes in his preferences, selecting as follows: B I *a*, II *b*, III *b*, IV *a*, V *a*, VI *a*; F I *b*, II *a*, III —, IV *a*, VI *b*. Other instances of fluctuations of this kind are given by nos. 21, 29, 37, 40, and 48. The results were:

21	29	37	40	48
B	B	B	B	B
I *a*	I —	I *a*	I *a*	I *b*
II *a*	II *a*	II *a*	II *a*	II *a*
III *a*	III —	III *a*	III *b*?	III *a*
IV *a*	IV —	IV *a*	IV *a*	IV —
V *a*	V —	V *a*	V *a*	V *b*
VI *b*	VI —	VI *a*	VI *a*	VI *a*
F	F	F	F	F
I *a*	I *a*	I *b*	I *a*	I *a*
II —	II —	II *a*	II *a*	II *b*
III —	III *b*	III *b*	III —	III *a*
IV —	IV —	IV *b*	IV *a*	IV *b*
V *a*	V —	V *b*	V *b*	V *b*
VI *a*	VI —	VI *b*	VI —	VI *a*

Already these five cases, when compared with the records for A and E, show that the modification, due to the black background, did not affect the preferences of all equally strongly or even in the same direction; and it is, therefore, not surprising to find that a great many subjects, as far as they belong to the positive weight-class, remained even totally unaffected by the changed conditions. Such are for instance: nos. 9, 11, 12, 13, 17, 38, 42, and 43, while nos. 14, 20, 26, 34, 38, 41, 44, 45, and 50 reversed their selections in only one or two out of the 12 plates of series B and F. It is interesting to observe that nearly all those who adhered to their original preferences, were strict and thorough-going followers of the weight-principle, and the more uncompromising their selections on the weight-principle were in A and E, the less changes did they show in B and F. The idea, therefore, naturally suggests itself to consider the records of B and F as index to the strength of the weight-idea, in such a manner that the absence of changes in B and F would indicate a decided consciousness of the weight-differences, undisturbed by the contrast-effects; the occurrence of changes, on the other hand, an unstable and weak weight-impression with the tendency to accept other criteria, such as might be offered by the changed conditions. A general statement of this kind would be quite admissible, if merely the *intensity* of the weight-impression is considered; but a correction must be added, if it is to take into account also its *qualitative*, and not simply its intensive differences. For greater sensitiveness to the balance of a figure would produce the same results as the relative absence of the weight-idea; *i.e.* it would show itself in similar fluctuations of preferences under the varied conditions of B and F, so that the objective results would in themselves offer no evidence of being due to either mere weakness of the weight-impression or to the greater sensitiveness to it. Apparent instability of the weight-impression as revealed in a certain irregularity in the results might, therefore, be referred to either of these two factors, and conformity to the rule and regularity in the objective preferences, though showing the intensity of the weight-impression, are not a proof of its delicacy or refinement.

3. This exhausts the evidence furnished by introspection. What remains is the testimony of the objective preferences, which, though not giving direct proof, at least point to a strong presumption in favour of some idea of weight, underlying the selections.

The first point is the evidence given by the results of series C, D, G and H. It will be remembered that these series (of squares and circles) were specially introduced to furnish conditions which might serve to disturb the direct and unimpeded action of the weight-impression, if such were really present in the triangle-series. Neither squares nor circles carry with them the same emphatic suggestion of verticality as the triangles, though not both to the same degree. The squares would naturally do so more than the circles, particularly owing to the usual over-estimation of the vertical over the horizontal extension. But the division of the squares in the diagonal, instead of in the horizontal line might, I thought, prove a sufficient counterpoise to balance this inherent advantage of the squares over the circles. In fact, since the line dividing the colours runs diagonally across the figure, it was now the circles in which the vertical extension predominated; for, mechanically speaking, the colours of the circles were in a position to act by their weight *vertically* upon each other, while in the squares the pressure could only be exercised laterally and in a slanting direction, and consequently not with its full force. I anticipated that, by reason of these modified conditions the weight-impression would be sufficiently weakened to allow no selections to be made on its account, but the results of Table III show that this assumption was false, at least partially. The weight-effect proved to be more delicate and impressionable than I had expected, and the majority of subjects arrived at some choice, usually with considerable precision and definiteness. The preferences, however—and this was the partial realisation of the forecast—are mostly very irregular, and the records present such a chaotic medley as practically defies analysis or classification. This in itself is, I repeat, a not unimportant fact, for the differences of opinion, even among adherents of the weight-principle, indicate the weakness of a single directive criterion, such as the weight-principle would have afforded, and betrays the action of several underlying influences introduced by the modified conditions and largely by the purely spatial element of the figures. From this point of view, therefore, the evidence of these series confirms the expected weakening of the weight-impression, and hence by implication its presence and activity in the triangle-series.

In some of the purest cases of the weight-class as in no. 17 or 50, the preferences in series C, D, G and H are throughout in perfect conformity with it, as can be seen by reference to Table IV. The idea of instability or top-heaviness made itself felt, as mentioned before, in the expectation of seeing the darker top part of the squares slide off the lower section, and the circles in *b* turn round into position *a* to find their balance. This in various modifications was the usual impression; no. 48 even said in regard to the squares that it seemed to him as if the dark top would fall down like a shutter, turning round the lower left-hand angle of the square as on a hinge. However, individual differences in the intensity of the weight-effect revealed themselves here, if anywhere; and the results accordingly vary between the two extremes of complete selections on the weight-principle (as in no. 17 and 50) and the (almost) complete absence of preferences, as illustrated by no. 22. The number of those who gave some preferences interspersed with a greater or smaller number of doubtful choices, and those who give full, though irregular records is nearly equal (17 to 23).

Conclusions similar to those drawn from the comparison of the results in A and E with those of C, D, G and H, as wholes, are arrived at by comparing the preferences in the square- with those of the circle-series. That the squares presented some particular difficulty is shown by the fact that, while there are but two cases where only one or two selections were made from the circles (nos. 22 and 41 respectively), there are eight subjects unable to select from the squares: nos. 4, 9, 10, 21, 25 (only in C), 30 (only in G) and 39 (only two preferences). In the light of the foregoing remarks there can be little doubt that this particular difficulty consisted precisely in the application of the weight-principle to the square-series.

In trying to account for all the preferences in these series in all their confusion and irregularity, the interest of doing so would hardly compensate for the tedium of wading through endless repetitions and uninteresting details. Some of the motives other than the weight-principle, which were especially predominant here, will be passed in review afterwards, but are out of place here, as being independent of the weight-principle. Only one point should be mentioned as partly explaining the confusion in these series, and that an objective feature which it would have been better to avoid, had I been aware of it from the beginning. It is one of the drawbacks of the otherwise advantageous method of approaching experimentally a problem without any previously formulated theory that the experimenter is apt unconsciously to introduce unnecessary complications. This is one such disturbing factor which made selection difficult and necessarily uncertain. There are several plates, as is shown by Table IV, where the luminosity-differences between the two colours is so slight as to cause some difficulty in estimating them by sight: in C III 29·83 as against 22·16, in G I 79·5 and 79·0, in D II 60·83 and 54·5, in H II 32·5 and 22·16, in III 54·5 and 52·0, and in IV 79·5 and 83·5. Generally small differences are fully appreciated, when found between shades of the same colours, but with tones even differences such as those in H II caused considerable embarrassment, not to speak of G I, where the two colours are to all intents and purposes of identical luminosity. Hence the constant irregularities in the preferences of G I, 10 subjects choosing *b*, while a great number got over the difficulty by not choosing at all. In C III there ought not to have been much uncertainty, nevertheless 15 subjects reserved their judgment; but in this case a good deal of the hesitation must be put down to the fact that C III belongs to the square-series, since the preferences of A IV, which contained the identical colours, shows little doubt or differences of opinion in the selection of *a*.

The other point, connected with the objective preferences and illustrating the action of the weight-idea in an unexpected way, is the record obtained with plates E II and III. That there must have been something peculiar about them is clear from the irregular manner in which selections from them were made, and I have already had occasion to mention that both these colour-combinations presented some difficulty as regards choice. A glance at Table IV reveals immediately the cause as far as E II is concerned. The colours were dark blue-purple and dark green with luminosities 87·16 and 86·16 respectively; for tones this difference was evidently too small to be estimated with certainty and the hesitation as to the position of the colours was therefore only natural, eight subjects again avoiding the difficulty by not choosing at all. (In F, on black, the difficulty seems to have been increased to judge by the slightly larger number of cases of indecision.) This result I had expected, having purposely selected as a test two colours which seemed to me to balance each other, and the subsequent determination of the luminosities showed the correctness of this anticipation. E II therefore offers no new features and might be grouped with the plates mentioned in the previous paragraph as presenting simply an objective difficulty to selection. Now E III had been devised for the same purpose as E II. Since at the time of selecting and arranging the colours I was not as yet definitely aware of the weight-principle being actually based on the luminosity-differences, I simply combined and adjusted the colours by sight and satisfied myself by my own impression that in E III the two colours (red-purple and green-blue) approximately balanced and might serve as a test-plate in the same way as E II. The results confirmed this belief. Nearly all subjects expressed their doubts about selecting either *a* or *b*, eight leaving the question of preference open. Of the remainder, eliminating those who acted on motives other than the weight-feeling, 24 voted for *a* and 13 for *b*. But my astonishment was great when, on determining the luminosities, I found that of the red-purple to be 81·5, as against 65·0 of the green-blue. Even making allowances for the fact that it was a question of tones and not of shades, the difference was still considerable, and there ought not to have been the slightest doubt, either for myself, when making the plates, or for any of the subjects, that *a* ought to have been selected. The number of preferences for *b* and the doubtful cases (together 21) appeared quite unaccountable, if the determinating factor was really constituted by the luminosities. It was only when measuring the triangles that I hit upon what I believe to be the solution of the riddle. While namely in E II, where the luminosities of the colours are nearly equal, the horizontal division-line between them was 7·6 cm. from the base, the distance in E III was only 7·0 cm.; so that in *a* of E III the blue top was rather higher than the purple base. That is to say, the areas covered by the colours differed very considerably in the two figures respectively; in E II 98·04 sq. cm. of blue-purple at the base, was matched against 43·12 sq. cm. of dark green at the apex; as against a red-purple base of only 90·30 sq. cm. with a top of 47·79 sq. cm. of green-blue in E III. These differences appear less trifling, when the relatively small area of the whole figures is taken into account; and the results show that even this comparatively small increase of the area of the lighter colour in E III was sufficient to make it doubtful to many subjects whether this triangle was properly balanced[1]. The purely accidental and unpremeditated nature of this difference makes the evidence which it offers for the sensitiveness and fine adjustment of the weight-impression all the more valuable; and, though it cannot absolutely prove its existence, it is one of the strongest presumptions in its favour which the objective results in themselves can afford.

III. So far the weight-principle. It remains to review as briefly as possible those criteria which became active, either side by side with, or as a substitute for, the idea of weight, owing to the individual idiosyncracies of the subjects or the disturbing influence of the spatial elements of the figures.

1. The first of these principles on which, independently of any weight-impression, selections were made is what is best included under the name of *landscape-associations*. By this designation is meant the

---

[1] I would like to make here a remark to prevent a verbal misunderstanding, which arises from the lax use of the word 'balance.' If two material substances of the same specific gravity are to be balanced (in the ordinary sense of the word), they must, of course, be taken in equal quantities. In the case of colours, their luminosity might be regarded as equivalent to the specific gravity of material substances, and to be balanced, they would be required to be also in equal quantities, *i.e.* covering equal areas. It might be objected therefore that in E II *a* 98·04 sq. cm. of one colour could not possibly balance 47·79 sq. cm. of another. This is, no doubt, true; but here it is not a question of horizontal, but vertical balance; instead of vertical balance it would perhaps be better to say stability. Stability is assured by the low position of the centre of gravity, so that it was not balance in the ordinary sense of the word that made a triangle acceptable, but rather the preponderance of the lower section. Now in E III *a* the increased area of the lighter colour at the top would naturally, in spite of its increase, not balance horizontally the lower colour. But it balanced it nevertheless vertically, that is to say, it counteracted the effect of 'bottom-heaviness,' if I may use this expression as converse to 'top-heaviness,' and thereby produced the impression of instability of the whole figure.

It is further to be remarked that the text as above shows nothing beyond this; in particular it shows no reason why E III *b* should have appeared acceptable to so many subjects. In *b* the areas were identical, but the colours reversed, so that it contained 90·30 sq. cm. of green-blue at the base and 47·79 sq. cm. of red-purple at the top. This, however, is very misleading as it stands, for in spite of its large area the green-blue base was vertically much smaller, *i.e.* lower than the apex of red-purple. As a result, since it was a question of stability, the figure appeared less top-heavy than it would have been had the base been even very much larger, *i.e.* had the distance of the dividing-line from the base been greater, because the top of red-purple was much longer and the centre of gravity placed lower than was the case in other plates. Hence the effect of top-heaviness was lessened, which I think partly accounts for the number of preferences for *b*.

Lastly it should be remembered that the results of these triangle-series cannot be directly applied to the effect of a wall painted two different colours, because the same spatial conditions do not obtain with a wall as with a triangle, since a wall possesses both horizontal and vertical extension in equal degrees, while in the triangle the vertical extension predominates so markedly over the horizontal.

association of a revived visual image of a land- or seascape with the actually perceived figure, or, more accurately expressed, the interpretation of the figure in the sense of such a land- or seascape or of part of it. Interpreting a triangle as any concrete material object, viz. as a sugar-loaf with its lower half wrapped round with paper, or as a pyramid, built of a darker and a lighter material, is not to be classed under landscape-associations, not so much because a sugar-loaf or a pyramid is not a landscape, as because there the characteristic of such associations is missing, namely the illusion of a perspective or the division of the figure into a fore- and a background. This illusion of the third dimension is the distinctive feature of this class. Landscape-associations were fairly numerous even among subjects who otherwise followed the weight-principle; already in no. 1 I met with them, and in one or two cases they almost became the dominant factor. It is interesting, however, that very many subjects were entirely free from them and that even, when the possibility of interpreting some plates in this manner was pointed out to them, they still confessed that they were unable to look upon them in that light. (Might it not be conceivable that subjects who formed these associations belong to the 'visual type,' and that the absence of visual imagery prevented others from seeing landscapes in the figures? Unfortunately I had no opportunity of specially testing the imagery of the subjects apart from the experiments.) These associations occurred most commonly in the circle-series and among these again most frequently in D III. This plate lent itself indeed most naturally to that interpretation, being composed of dark and light blue and thus forming in *a* a circular picture of the sea with a pale-blue sky above, the division-line representing the horizon. Wherever associations were met with, this was one of them, being found in nos. 1, 11, 12, 18, 21, 25, 26, 28, 35, 37, 40, 43 and 48; and knowing from other experiments the obstinate persistency of landscape-associations which have a tendency to monopolise everything and to become a perfect obsession to the subject, I took care after the first few tests never to show this plate until quite at the end of the series. For if once the possibility of constructing a landscape out of a figure is discovered, there is no end to it and every combination of colours, however unnatural, is treated on the same pattern, as long as with the wildest stretch of imagination a landscape can be made out of it. Among the square-series, landscape-associations never occurred, with one solitary exception, namely no. 40 in C II (dark green and light green), which both in *a* and *b* appeared to him as the slope of a hill with a dark or light sky behind it, in spite of the fact that neither the dark nor the light green was a suitable colour for any sky. The only really noteworthy feature about these otherwise uninteresting cases was the occurrence of landscape-associations with the triangles. There were only four cases of it, namely no. 18 (A I II, B IV, E IV), no. 21 (A I II IV VI, B I II IV VI, E III IV), no. 37 (A III), and no. 40 (A I II), and Table III shows that these associations produced selections both of *a* and *b*, but mostly *b*'s, which accounts for some of the apparent irregularities of these records. Both nos. 18 and 21 agreed in stating that in their opinion the triangular shape of the figures was in itself conducive to landscape-associations by suggesting the idea of perspective, the reason being, I suppose, that the attention naturally travels from the base of the triangles to the apex in the same way as it extends from the fore-ground to the background of a landscape. But the absence of associations in the triangles with the majority of other subjects who formed them with the circles, shows that this is only an individual peculiarity of these few exceptional instances.

2. Considerably more numerous were selections made on the basis of a preference for one of the two colours composing a figure. Even in cases following the weight-principle this criterion was occasionally applied, especially in its negative aspect, *i.e.* one figure was rejected, because containing a larger amount of an unpleasant colour than the other. This was notably the case with plate E VI (yellow and bright blue), where *b* was chosen as containing less of the objectionable blue. Sporadically instances of such colour-preferences are to be found in the greater part of all the records; I simply noted them without making any attempt to obtain an explanation of the preferences themselves, which I considered as lying outside the range of the tests. In one case at least (no. 7) *all* preferences throughout all the series were made on this principle (which accounts for the confusion in that record) and in several other instances the greater part of the selections is due to the same reason, as in nos. 22 and 47. The most interesting point about this class is that it falls into two divisions which are fairly constantly separated. The more usual course was to select that triangle in which the preferred colour occupied the larger area, that is to say the base, "because there was more of it." This would, I think, represent the aesthetically crudest form of criterion used. The other, however, is distinctly superior and shows a far finer aesthetic appreciation; it consisted in selecting that figure in which the preferred colour was

placed in the apex of the triangle. Though covering a much smaller area than the other colour, it was felt to receive a special prominence by occupying the top of the triangle, which is its most characteristic feature. Selections of this kind show, therefore, not simply the crude motive of *quantity* of the preferred colour, but reveal a marked appreciation of the qualitative differences of its position and a sense of its spatial application. Instances of this manner of choosing are: nos. 16 (A IV V VI), 24 (A V), 29 (A III), and 40 (E IV). Selections of the square- and circle-series on this principle of colour-preferences, one would have expected to be theoretically impossible, since both figures in these series contained both colours in equal quantities. They were however also made, though in limited numbers, involving often illusions presently to be mentioned.

3. The final point to be noticed is two forms of optical illusion which I simply mention, because I am unable to offer any explanation of them. They were largely instrumental in deciding doubtful cases, though in some instances they remained apparently without influence on the preferences and were merely incidentally noticed. They comprise either *apparent differences in identical colours* according to their position in the figures, or *apparent differences in the shape of identical figures* according to the position of the colours of which they were composed. The first of these illusions is represented by nos. 3 and 24, who both stated that in G I (square, red and blue) the red appeared less dazzling in *b* than in *a*, while no. 15 maintained that the blue seemed less glaring in *b*. Objectively neither the one nor the other difference between the colours can have existed, since both colours were cut from the same strips of paper. Similarly in regard to C II both nos. 20 and 26 agreed in saying that the dark green appeared darker in *b* than in *a*; no. 30 said that in E II the blue-purple was less glaring in *a*, and the red in E IV darker in *b*; though in all these cases, for the same reason as before, these differences cannot have had any objective foundation. Other cases are: nos. 19 (H II), 26 (G II), 29 (E I VI), 31 (G III, A IV), 34 (E I), 41 (D II), and 42 (H II). Being found mostly in the squares and circles, these illusionary differences furnished a welcome motive for selection, where the weight-impression failed.

Under this same head might also be classed a very obscure statement which seems similarly to depend on the position of the colours relatively to each other, namely that in one of the two figures the colours "blend better," with the result that this figure is selected. What this "blending" consisted in, or what was actually meant by it, I have not been able to find out; no. 16 also used the expression that the colours were "more confluent." The number of subjects who noticed this feature was very small: nos. 16 (E I *a*, D I *a*, H I *b*), 18 (E II *b*, G II *a*), 29 (E V *a*) and 41 (A I *b*, D II *b*); if it were not for the presence of H IV *b* and A I *b*, one would be inclined to take this "blending" simply as a periphrase of the weight-impression, *i.e.* as the result of that order of the colours which was felt to be the most natural (E II *b* being in any case a doubtful plate); but as it is, this explanation is somewhat questionable.

Equally unaccountable was the reason given by no. 16 for his selection of C III *b* and D II *b*, in which, he said, the contrast between the colours was more marked in *b* than in *a*, unless this again means that the inversion of the natural sequence of the colours produced a more pronounced contrast in *b*. But being the only instance of this reason, and therefore, without parallel cases, it is also an obscure point.

The second form of illusion concerns the shape of the figures, and refers exclusively to the series D and H, thus offering a criterion for selection from these figures, which, apart from the weight-principle and landscape-associations, would all have been equivalent to each other. I have mentioned already earlier the usual optical illusion, that, owing to the diameter being visible, the circles appeared as horizontal ellipses; but they did so in varying degrees, sometimes *a*, sometimes *b* seeming to be more flattened at the poles than the other circles. Thus to no. 3 H I *b* and III *a* appeared flatter than I *a* or III *b*, while to no. 22 H I *b* seemed rounder than *a*. In a similar way figure *a* often appeared larger than *b* to some subjects: nos. 20 (D IV), 26 (D I, H I IV), 27 (D IV, H I, IV), 30 (D I, IV, H I). Here again it is hardly possible that an objective reason can have existed to render the impression anything but a subjective illusion.

And this is especially true of the most important and striking illusion which occurred in the circle-series, namely the *illusion of relief* or *convexity*. It was a rule without exception that in many of the circles, *b* produced the effect of a ball or globe, while *a* in that case always appeared as a flat disk. This accounts for the very frequent preferences for *b* in D and H, even in cases which otherwise adopt the weight-principle. If the plates were turned 180°, the effect still persisted, but then the original *a* appeared in relief, so that the illusion is quite undoubtedly due to the position of the darker colour above the lighter one. Not every subject experienced this illusion, but a fair number noticed it: 16 (D IV, H I), 18 (H I), 20 (D I, H I), 22 (H I), 36 (D I II, H IV), 31 (H I), 39 (D II IV, H I), 42 (D I III, IV, H I), 44 (D IV), 46 (H I II, D IV), 47 (D I II III) and 48 (D I II IV, H I IV). I am unable to offer any explanation, why the dark colour above should produce this impression, nor have any of the subjects suggested any possible ground for this effect.

This concludes the list of principles or criteria which were operative in the selections, as far as they have come to my notice, and I believe that with their help most of the preferences, recorded in Table III, are explicable. As illustration I subjoin here the analysis of one of the most complex cases, uniting nearly all the varieties of motives which occurred more or less separately in other instances. It is no. 18:

A		E	
I *b*	landscape	I *a*	weight-impression
II *b*	do.	II *b*	better blending
III —		III *a*	weight-impression
IV *a*	weight-impression	IV *b*	landscape
V *b*	preference for pink	V *a*	weight-impression
VI *a*	weight-impression	VI *b*	less blue in *b*
C		**D**	
I *a*	weight-impression	I *b*	appears higher than *a*
II *a*	do.	II *a*	weight-impression
III *a*	do.	III *a*	seascape
IV —	do.	IV *b*	landscape
G		**H**	
I *a*	weight-impression	I *b*	relief
II *a*	better blending	II *b*	colours not in same plane [in *a*
III *a*	weight-impression	III *b*	relief?
IV *a*	do.	IV *b*	more restful than *a*

4. *Conclusion.*

The point that darker colours appear heavier than lighter colours being decided, the question, *why they should do so*, still awaits an answer.

In the course of the tests I asked several of the subjects what in their opinion the reason for such an impression might be. The majority admitted having never considered the question; and the rest, if they were disposed to suggest a reason at all, mostly answered that it was "due to association." So undoubtedly it is; but the word 'association,' in spite of its supposed omnipotence to solve all problems, brings hardly any enlightenment. For the point is: what is it that is associated with the colours to produce the effect of heaviness? Those subjects who ventured beyond the statement that its cause was association, suggested that, being constantly surrounded by natural objects which were of a darker colour below than above, we had contracted the habit of considering this arrangement as typical and expected to find it conformed to also in objects which, as the triangles for instance, do not form part of our customary environment. That is, custom has bred in us the normative idea that darker colours should stand below lighter ones, and the lower part of an object being generally heavier (to preserve stability), the impression that dark colours are heavier was the joint outcome of habit and inference. Trees for instance, one said, are always darker at the base than at the crown, and a mountain, another suggested, shows the same feature: in short, it is the association of visual memory-images of natural objects with the perceived colour-arrangements that produces the weight-effect of darker colours.

There are two objections to be made to this explanation. Firstly the correctness of the assertion that natural objects, possessing the particular colour-arrangement in question, are to be found in sufficient quantity to allow the necessary habit to become so fixed, that it is at once and quasi-instinctively applied to any other object, however widely different from natural objects both in shape and colouring. That trees, for instance, are darker at the base than at the crown, apart from the changing shadows cast by the foliage, seems to me very questionable. Mountains, especially high ones, are indeed of a fainter colour at the top than at the foot, owing to aërial perspective; but the difference is relatively small, and not many people are likely to be sufficiently impressed by it, to deduce a normative idea from it. Many instances of natural objects, as the light sky over a wood or the sea, in fact over any landscape, are not admissible, though presenting the required combination of two colours in the most constant form; for the landscape and the sky appear as two different things, situated in different planes, and not as mere qualitative differences of one flat surface, as the triangles appeared in the tests, as long as landscape-associations were absent. Looking about us in nature, the colour-arrangements seem quite accidental. We may find a corn-field in the fore-ground with a dark wood behind it quite as often as a meadow with a corn- or clover-field beyond; there are white cottages with thatched or tiled roofs and flower-borders with dark shrubs behind them. I have since tried to find a sufficient number of natural objects exhibiting the required colour-combination with some definiteness and constancy, but I have not been able to convince myself as yet that the preference for darker colours below lighter ones can in any way be referred to a habit contracted from our environment.

The second point which speaks very strongly against an association with visual images of natural objects, is the curious antagonism in which the weight-principle seemed to stand to the landscape-associations in the tests. The two were never, except in a very few cases, applied to the same figure by the same individual; in the few exceptional instances, when it was done, it always produced opposite results. The two methods of selection represented evidently two entirely different mental processes; the one was the apperception of a geometrical figure as figure, the other the interpretation of the figure as possessing a 'meaning,' as representing something else than a mere figure, namely a landscape. As no. 40 said in the case of C II: "If I take this to be the slope of a hill, I have no preference; if I take it simply as a figure, I prefer *b*." The two principles, the weight-principle and the landscape-association, seemed to be irreconcilable.

If the reason of the apparent weight of darker colours is not due to the association of visual images with the colour-sensation, what is the reason? Suppose there are two stones, one twice as big as the other; merely looking at the stones, we say the second "looks" heavier than the first. Why? Because this stone is larger, has more volume or substance than the first; in short there is "more of it." Or, to use another analogy, if we have a glass containing a weak mixture of claret and water and a glass with a strong mixture before us, the second will appear to be heavier for the same reason, because it seems to contain more substance *i.e.* more claret than the first. I do not mean that it need actually *be* heavier, but that it only "*looks*" heavier. Just the same happens with colours. If we see pink and red side by side, the red looks heavier than the pink; why? Because there is, or there seems to be more substance in it than in the pink, there too is "more of it," not in the sense that it covers a larger area, as the bigger stone does relatively to the smaller, but in the sense that it contains more pigment, just as the strong mixture of claret and water contains more claret. Or to express it differently: if more pink were added to the pink, it would eventually become red, just as, if one added more stone to the smaller stone it would ultimately become as big as the large one, or as by putting more claret into the mixture, it would finally become as dark as pure claret. So far the unsophisticated conception of the matter is right. If one adds more red to the pink, it will become redder and redder until it is a fully saturated red. At the same time the luminosity will decrease, *i.e.* the colour becomes darker, and the increasing darkness appears as the result of an addition of pigment to the original pink. But the luminosity which has continually fallen with the addition of pigment, can be decreased beyond the point of saturation, producing a still darker red. This again appears as the result of a further addition, not indeed of pigment, but of black: dark red therefore, compared with saturated red, has again this peculiar quality of 'more-ness,' it has again more substance, *i.e.* it appears heavier. Hence luminosity, not saturation is the true index of the weight of a colour.

There is, I think, little doubt that no one to whom a colour looks heavy, actually puts to himself the question: "of what is there more?" Whether the apparent increase of substance is due to the addition of pigment or of black, matters little to the weight-effect. Most people would probably say, it was, in *any* case, an increase of pigmenting substance; the main point is, that "there is more" of whatever it may be, and the effect is that of weight or consistency, density, solidity or impenetrability in all its various modifications.

That this effect is due to association is undoubtedly true, though not in a sense of an association of a visual image with the colour-sensation, but of an association of ideas of weight with the visually perceived object, *i.e.* a complex originating from two different sensational spheres, just as the 'look of heaviness' of a stone is such a complex. While on the one hand, it is quite unintelligible, how the casual reminiscences of natural objects could produce such a delicate discrimination as was observable in the tests, between the weights of colours which are perhaps even rarely met with in ordinary surroundings, nothing, on the other hand, could be more natural and obvious than these finely graduated impressions of differences on the basis of such a complication, which we have occasion to practise in various forms a hundred times a day. That the attribution of weight to colour is not made by means of any conscious inference, but is purely unreflective and immediate, need hardly be remarked; as the stone, so the colour simply "looks" heavy.

The weight-idea is naturally most marked, when relative, *i.e.* when it is the result of the weight-differences between two colours seen side by side. But it also exists *absolutely*, *i.e.* in face of only one colour. Since the saturated colours are not all of the same luminosity, but yellow and green are of higher luminosity than blue and violet, yellow and green are also of apparently lighter weight than blue or violet, when seen singly. In a similar manner as the movement, tension and balance of geometrical figures, attributed to them or '*eingefühlt*' into them, form the basis of an interpretation of them as individual organisms, possessed of an inner life and a kind of distinctive personality, so the weight-impression of colours, at first merely designating, as it were, a physical aspect of them, is the intermediate step to the apprehension of colour-effects which are not inherent in colours as objective realities. A golden yellow *looks* light and it *is*, as an aesthetic reality, cheerful, energizing; a rich blue *looks* heavy and it *is* serious, easily even depressing. No doubt, in such an aesthetically anthropomorphic 'realisation' of colours the idea of weight plays only a small, even very small part (the main part falling to the share of the tone-element), but it is just this relatively insignificant contribution to aesthetic colour-effects that I wish thus briefly to point out, and thereby the connexion of the weight-impression with, and its bearing upon, the Aesthetics of Colour generally.

In one of his earlier papers Kraepelin says, "Modern school claims are beyond possibility of accomplishment. Our children would of necessity succumb to chronic mental disease were they compelled to work for forty minutes per lesson throughout the day at full tension of attention. The fact that only a few are seriously injured is due to the subjects taught and to the teachers who permit the scholar the opportunity of loosening the reins of a worn out attention." Such emphatic language from one who has made so careful a study of the subject, places the question of mental fatigue in the very forefront of those with which modern pedagogy is confronted, and calls for far more attention to a condition, the effects of which are all too well known to those who are engaged in intellectual pursuits.

In Germany and France a growing interest in the subject has led to careful study, and to experimental investigations, but in this country exceedingly little has been undertaken. On the Continent tests have been applied in various psychological laboratories to adult subjects (University students, medical men, etc.), who have been tested singly, or in small numbers, but with great care and exactness, as well as in schools where they have been set to classes often of considerable size, generally by some interested visitor. In the latter case, undoubtedly much of that attention to absolute details which is so necessary in scientific research has been unattainable. But there can be no doubt that it is in this direction that the successful investigation of the subject lies, after the way has been pointed out however by the more exact laboratory practice. In fact it is only after the more theoretical problems regarding the psychological effects of intellectual work have been at least to some extent solved, and the most suitable methods developed and tested under the rigid conditions of laboratory research, that the application of such experiments to large numbers of subjects can be of real service. For while on the one hand, the testing of isolated individuals can never yield conclusions of general value, on the other hand the results obtained from indiscriminate masses of subjects, whose health, home conditions, state of nutrition, and numberless other important factors, may vary from day to day, are equally uncertain. It is owing to these almost insuperable difficulties, combined with the enormous trouble of fitting in suitable experiments without disturbing the usual routine of school work, or being in any way disturbed while the experiments are in progress, that this side of the practical investigation of mental fatigue has received up to the present in this country, an amount of attention altogether incommensurate with its importance.

Oehrn in his *Experimentelle Studien zur Individualpsychologie*[1] made it quite clear that with differing tasks the same individual behaved differently, and he further indicated that striking correlations existed between the effects of practice and fatigue as exhibited in particular groups of similar types of mental work. For instance, he found there was a distinct resemblance between the classification of his subjects according to work done at memorising figures and that according to work done at memorising nonsense syllables, but these two differed altogether from those obtained for counting letters, addition, and dictation, three forms of work which on the other hand exhibited striking similarity of results among themselves. This branch of the subject has received, however, comparatively little attention. Oehrn considered it from the point of view of speed of work but did little else. In the experiments of which this paper is a brief account an attempt has been made to carry investigation of the question of the relative effects of fatigue and practice upon different varieties of mental work a little further.

I. (*a*) *Method and arrangement of experiments.*

For the purpose in view Kraepelin's *Rechenhefte* (books, the pages of which contain a number of parallel columns of digits) were employed, and four kinds of mental work, all of the nature of mental arithmetic, were employed. In the first research, carried out during the Spring of 1905 the tests consisted of, (i) [*A*] Addition of two digits continuously, thus:

8	:		
3	:	1	$8+3=11$
5	:	8	$3+5=8$
7	:	2	$5+7=12$
9	:	6	$7+9=16$
4	:	3	$9+4=13$

The subjects added as shown, and wrote only the unit figure of the sum down, in order to reduce the physical effort of writing as far as possible.

(ii) [M_3] Multiplication of three digits as follows:

8	:	
3	:	
5	:	0
7	:	5
9	:	5
4	:	2

---

[1] Kraepelin's *Psych. Arbeit.* Bd. I. S. 92.

The subjects were directed to multiply 8 by 3, then the unit figure *only* of the product by 5, and write down by the side of the 5 the unit figure of the product. The mental process in this case would therefore be: "$8 \times 3 = \cancel{2}4$; $4 \times 5 = 20$; write down 0." Similarly for the next figure, "$3 \times 5 = \cancel{1}5$; $5 \times 7 = 35$; write down 5." For the next figure, "$5 \times 7 = \cancel{3}5$; $5 \times 9 = 45$; write down 5." The products were written in pencil as shown above.

In the second research, carried out during the same term of the following year, but unfortunately of necessity on different subjects, the tests consisted of: (i) [M_2] Multiplication of two digits, in precisely the same way as the addition was performed in the earlier tests. (ii) [M_4] Multiplication of four digits as follows:

$$\begin{array}{c|c} 8 & \\ 7 & \\ 9 & \\ 3 & 2 \end{array}$$

The first figure was multiplied by the second, the unit figure only of the product multiplied by the third figure, and the unit figure only of this product multiplied by the fourth figure. Thus, to take as an example the figures shown, the mental process involved would be $8 \times 7 = \cancel{5}6$; $6 \times 9 = \cancel{5}4$; $4 \times 3 = \cancel{1}2$; and the figure 2 written opposite the 3.

We shall in future refer to these various tests merely by the four distinctive signs, A, M_2, M_3, and M_4 respectively. But we can hardly institute any comparisons between the results of the two investigations since the same subjects were not employed, and further, although it will be at once seen that the M_3 work involves a much greater mental effort than the merely mechanical processes of the A experiments, and the M_4 tests than the M_2, it is extremely doubtful whether the ratio of degree of difficulty is the same between the M_2 and M_4 work as it was between the A and M_3 work of the earlier research, and this must be borne in mind when the results are considered. All that can be affirmed on this point is that in both cases an easy and a much more difficult task were set; beyond this we can hardly go since both the nature of the tasks, and the subjects tested, varied.

As subjects, twelve boys of an Upper Fifth form were chosen in each case, who had been under the charge of the author for mathematical subjects for the previous three years, and thus were thoroughly well known. They were selected by no means for their ability, but according to known regularity of home life, sleep, nutrition, and above all general reliability. Their ages varied between fourteen and sixteen years, and as there was nothing novel about either the work they had to do, or the general character of the experiments, the results, it is felt, give a very good indication of their regular working capacity, such as would hardly have been attainable with some more complex method, or with tests conducted by a stranger. To accustom them better to the idea of what was required, a very short introductory exercise was given during the week preceding the first of the series. Of course with children even of from fourteen to sixteen years there is always the danger of an abnormal concentration of attention during the earlier tests, followed by a gradual falling off of interest and attention, as the experiments proceed, and a consequent falsification of results. To meet this, prizes were offered to the boys who worked the most consistently throughout, and awarded at the conclusion of the tests.

The first research which took place during the Spring Term of 1905 was held on two mornings each week (Tuesday and Thursday) with one exception, and between the hours of 9 and 10. It consisted of eighteen experiments in all, divided into three identical series of six. In each series were three addition and three similar multiplication tests of the following character:—

A 0 Two consecutive addition work periods each of ten minutes, *i.e.* a pauseless experiment.

A 10 Two addition work periods of ten minutes separated by a rest pause of ten minutes.

A 20 Two addition work periods of ten minutes separated by a rest pause of twenty minutes.

M_3 0 Two consecutive multiplication work periods each of ten minutes.

M_3 10 Two multiplication work periods of ten minutes separated by a rest pause of ten minutes.

M_3 20 Two multiplication work periods of ten minutes separated by a rest pause of twenty minutes.

During the rest pauses the boys were allowed to chat with one another, or walk about, just as they pleased. Finally as large a variety of intervals as possible was arranged between the experiments, and no fixed order adopted, so that the whole scheme of the research was worked out as follows:—

1st Series ...	M_3 0	A 0	M_3 10	M_3 20	A 10	A 20
2nd Series ...	M_3 0	A 0	A 10	M_3 10	A 20	M_3 20
3rd Series ...	M_3 20	A 20	M_3 10	A 10	M_3 0	A 0

The second research which took place during the corresponding period of the following year, was conducted on precisely similar lines both as regards choice of subjects and the nature of the experiments, except that the work was arranged so that the interval between the two tests of the same type remained constant throughout. The M_2 tests were therefore held every Monday morning, and the M_4 tests every Tuesday morning, in each case between 9 and 10 A.M., throughout the Term. As before, the research consisted of three series of six 20′ experiments, three of each kind, of which one was pauseless, one involved a 10′ pause, and one a 20′ pause. The plan was therefore as shown:—

1st Series ...	M_2 0	M_4 0	M_2 10	M_4 10	M_2 20	M_4 20
2nd Series ...	M_2 0	M_4 0	M_2 10	M_4 10	M_2 20	M_4 20
3rd Series ...	M_2 0	M_4 0	M_2 10	M_4 10	M_2 20	M_4 20

From this it will be seen that an interval of one week separated each test from the succeeding one of the same type. An effort was made to diminish the effect of the spurt which usually occurs when the end of a test is in sight, by sometimes running the experiments on for longer than ten minutes at the end of the test and ignoring the final minutes. This did not apply in the case of the first half of the work periods, as the subjects never knew whether they were doing a pauseless experiment or not, whereas in the latter half they always had an idea when the end of the time was approaching. The prevalence of spurts, as Hylan's work has shown, is a probable source of error in short experiments, hence this attempt to guard against it.

(*b*) In both researches the main points to which investigation was directed were:—

1. The value and difference of the pause effects, and the determination of the more favourable pause in the two cases. It was expected that the harder task would generate the greater fatigue after ten minutes' work, and that in this case a pause of twenty minutes would be more beneficial than one of ten minutes. On the other hand ten minutes might suffice in the case of the easier work since a twenty minutes' rest would involve a disproportionate loss of Incitation[1], and therefore prove unfavourable.

2. The general relations between the work of the various individuals at easy and at harder tasks respectively.

3. The relations existing between improvability and fatiguability, and retentivity of practice, in the various subjects, and the extent to which these relations depended upon the kind of work which was done.

---

[1] *Anregung*, the process of getting up speed, or getting under weigh.

4. The possibility of obtaining, by means of introspective notes, information regarding (*a*) the kind and amount of fatigue experienced with the two tasks, (*b*) the liability to distraction observed in the two cases, (*c*) the presence or absence of muscular strain, eye-strain, or headache.

II. FIRST RESEARCH.

It will undoubtedly be less confusing, and much more convenient if we deal with the two investigations separately; we will therefore pass at once to a brief consideration of the earlier research, and endeavour without too minute an examination of the data, to show to what extent the objects of the experiments have been attained.

The results were noted minute by minute, instead of every five minutes as in Kraepelin's earlier experiments, otherwise the plan adopted was the same, and Table I is an example of how the data were arranged.

TABLE I.

3rd Series. Multiplication.

Subject	III. 13/4/05			II. 6/4/05			I. 29/3/05		
	M_3 0		Gain °/o	M_3 10		Gain °/o	M_3 20		Gain °/o
P ...	230	237	3·04	239	286	19·6	250	278	11·2
W ...	258	241	-6·5	198	246	24·2	219	186	-15·07
G ...	303	310	2·3	301	306	1·6	293	278	-5·1
C ...	334	344	2·9	316	322	1·9	330	342	3·3
S ...	278	269	-3·2	267	334	25·09	268	275	2·6
Y ...	313	355	13·4	379	415	9·5	371	371	0
D ...	367	378	2·9	359	381	6·1	390	376	3·6
L ...	406	400	-1·4	401	458	14·2	429	443	3·2
B ...	364	394	8·2	344	353	2·6	378	369	2·3
H ...	271	263	-2·9	236	263	11·4	269	270	0·3
F ...	454	444	-2·2	412	435	5·5	415	427	2·8
M ...	301	328	8·2	306	343	12·09	333	323	3·0

The three experiments are indicated by the Roman numerals, and the date. In the first column of each is shown the number of multiplications done by each subject in the first ten minutes, in the second column the number done in the second ten minutes, and in the third column the increase per cent. which the latter shows over the former. In no case have mistakes been deducted as these were found to be exceedingly few, and they could therefore have no appreciable effect upon the results. Similar tables were drawn up for the three series of addition and the first and second series of multiplication tests, but the inclusion of all the tables of data would add so considerably to the length of this paper, that such a course is hardly possible or even desirable.

(*d*) *The more favourable pause and the effect of Incitation.*

From the tables of which the above is a type it is a simple matter by merely noting the percentage increase after the rest-pause, to determine the more favourable pause. The results obtained brought several striking facts to light. For instance, in the first place, identical results did not occur in the three series. This might have been expected from the fact that the mental condition of the average schoolboy must change as the term progresses, particularly towards the end when examinations, and the subsequent holidays are in view. Then in the first series (addition) in no less than four (possibly five) cases, viz. *G*, *D*, *B*, *F* and *M*, the pauseless experiment was more favourable than either of the others. The same phenomenon occurred with *C* (addition) and *G*, *Y* and *B* (multiplication) in the third series, and with *M* (multiplication) in the second series. There seems no clear explanation of the last mentioned case but the others are by no means inexplicable. Those experiments of the third series which are concerned were held upon the two last days of the term—immediately before the holidays, that of the first series, during the first week of the tests, so that it is highly probable that these abnormal results are the consequences of a high degree of will-tension such as was not manifested upon other occasions. An enormous increase which *C* showed in his *A* 0 test of the third series, out of all proportion to his previous figures, is hardly to be explained upon any other hypothesis, and there seems no reason to doubt that the same factor produced similar results in the other cases.

Such abnormalities as this are bound to arise in any experiments of this nature which have of necessity to run concurrently with ordinary school work. It is due to this indeed that with respect to the pause effects, the second series is by far the most irregular, since the whole of this series was carried out in the three weeks immediately preceding the school examinations, and the subjects must therefore have been under the effect of a considerable degree of general fatigue. But notwithstanding this fact Table II, which gives the averages of the percentage increases, shows clearly that for both kinds of task the shorter pause is in the majority of cases the more favourable,

TABLE II.

Pause			Pause		
A	10′	20′.	M_3	10′	20′
P ...	*30·6*	28·84	......	*23·9*	10·2
W ...	*4·7*	2·7	......	*17·6*	-1·8
G ...	*8·75*	0·2	......	*9·5*	-1·5
C ...	*2·6*	-0·84	......	*8·7*	7·9
S ...	5·86	*8·95*	......	*20·1*	10·4
Y ...	*13·6*	12·7	......	*16·2*	6·9
D ...	*14·5*	13·2	......	*5·5*	0·7
L ...	17·7	*22·7*	......	*19·2*	8·5
B ...	*10·9*	5·7	......	*15·5*	8·5
H ...	*4·75*	1·8	......	*5·1*	3·3
F ...	*11·6*	10·3	......	*14·4*	5·7
M ...	7·1	*11·9*	......	*11·5*	1·4

but more frequently so in the case of the multiplication than in that of the addition. This is by no means what one might have expected, and points to the influence of incitation as opposed to mere practice. There can be little doubt that a greater degree of incitation is necessary, and is developed, in the more difficult task than in the easier one. Now during the rest-pause this begins to dwindle away, and probably in the case of the multiplication while a part remained after 10 minutes, practically all had disappeared after the longer pause. Hence in the latter case the process of 'getting under weigh' had to be gone through afresh, and consequently the work began at a lower level than would have been the case had the pause been shorter. The 'attunement' theory put forward by Hylan and Kraepelin[1] seems also to receive support from this result since there is every reason to suppose that during the short pause the subjects' thoughts, already attuned to their task, would hardly be as much diverted as they would be during the longer one. With the easier addition, however, the case is different. A smaller degree of incitation is present, and this in all probability disappears even during the shorter pause and so does not enter into the question of the relative values of the recovery effects. It therefore by no means follows that the more difficult the work undertaken, the longer will be the pause that will yield the maximum recovery effect. In addition to the wiping out of fatigue, and the loss of practice we must also take into account the degree of incitation which is involved.

---

[1] *Psych. Arbeit.* Bd. IV. S. 454.

(*b*) *Absolute amount of work done.*

As the method of estimating absolute output of work should certainly not involve pause effects, all work done after rest-pauses was omitted in the calculations, so that in each series only the pauseless, and the first halves of the other tests were included. The subjects were then classified as shown in Tables III [*A*] and IV [M_3] according to their positions with reference to the general average. Here again, however, the actual figures are not given for the reason stated above.

TABLE III.

	Good	Indifferent	Bad
1st Series ...	*P L D F*	*B Y M W C*	*S G H*
2nd ,, ...	*B D F L*	*Y P H G W*	*S M C*
3rd ,, ...	*L F D B*	*Y G M W S*	*H C P*
Total ...	*F L D B*	*Y W P G M*	*S H C*

It will at once be seen that there is very considerable agreement between the two tables, although of course the absolute amount done was greater in the case of the addition than it was in the case of the multiplication. Thus, whereas subjects *F*, *L*, *B*, *D* and *Y* come out high in both, *H* and *S* are low. If we examine the results in greater detail, we find, however, that this regularity is not maintained from series to series, for so far as absolute output of addition is concerned, Table III reveals a remarkable fall in the case of *P* who stands at the head of the list in the first series but falls to the lowest position in the third series. *F*, *L*, *D*, *B* and *Y* show consistently good work, and *S*, *H* and *C* consistently poor work throughout.

Table IV shows, as did that for the addition tests, a most remarkable fall in the case of *P*, with consistently good work on the part of *L*, *F*, *D* and possibly also *B*, but consistently poor work on the part of *S*, *H* and *W*.

TABLE IV.

	Good	Indifferent	Bad
1st Series ...	*L D F P*	*M Y B G H*	*C W S*
2nd „ ...	*F L D Y*	*B M P G S*	*H S W*
3rd „ ...	*F L B Y*	*D C M G H*	*S P W*
Total ...	*L F D*	*B Y M C G P*	*H S W*

These facts may be further tested by estimating the correlation which exists between output of addition and output of three-figure multiplication by Spearman's 'foot-rule' method[1]. For twelve subjects the probable error works out at ·12. The result yielded is ·58 (*i.e.* nearly five times the probable error), so that there is distinct justification for the assertion that those subjects whose output of work is highest at the easier task, will also do most at the harder one, provided the tasks are similar in character (cf. Oehrn, *op. cit.*).

(*c*) *Improvability.*

Two methods were employed for the estimation of improvability.

(*a*) In the first place, the work done during the three pauseless experiments in addition was taken, and (α) the total for the second series expressed in percentage increase over that of the first series, (β) the total for the third series expressed in percentage over that of the second series, and (γ) the total for the third series expressed in percentage increase over that of the first series. The results of these are shown in Table V, together with the average of α and β, and the position of each subject according to this average.

(*b*) Hylan, in his *Über die Wirkung kurzer Arbeitszeiten* gives another method. According to this, the second five minutes' work of every experiment is taken in actual time order, as a basis for the calculation of improvability. This method possesses the great advantage of avoiding, both initial spurt, and to some extent, the effects of developing incitation, and the author has therefore followed this plan. Each five minutes' work was in the first place expressed as percentage increase over that of the corresponding period of the preceding experiment. This procedure however yielded little advantage, and therefore,

---

[1] *Brit. Journ. Psychol.* Vol. II. p. 89.

TABLE V.

Improvability. 1*st Method.*

Addition. 0 *pause.* °/ₒ *increase.*

Subject	$\alpha \frac{II}{I}$	$\beta \frac{III}{II}$	$\gamma \frac{III}{I}$	Average of α and β	Position
P	$\frac{1253}{952}$ 32 °/ₒ	$\frac{704}{1253}$ - 44 °/ₒ	$\frac{704}{952}$ - 26 °/ₒ	- 6 °/ₒ	12
W	$\frac{967}{701}$ 38 ,,	$\frac{1122}{967}$ 16 ,,	$\frac{1122}{701}$ 60 ,,	27 ,,	8
G	$\frac{969}{623}$ 56 ,,	$\frac{1183}{969}$ 22 ,,	$\frac{1183}{623}$ 90 ,,	39 ,,	5
C	$\frac{715}{738}$ - 3 ,,	$\frac{1032}{715}$ 44 ,,	$\frac{1032}{738}$ 40 ,,	20 ,,	11
S	$\frac{955}{589}$ 62 ,,	$\frac{1030}{955}$ 8 ,,	$\frac{1030}{589}$ 75 ,,	35 ,,	7
Y	$\frac{1021}{772}$ 32 ,,	$\frac{1217}{1021}$ 19 ,,	$\frac{1217}{772}$ 58 ,,	25 ,,	10
D	$\frac{1369}{806}$ 70 ,,	$\frac{1473}{1369}$ 8 ,,	$\frac{1473}{806}$ 83 ,,	39 ,,	4
L	— —	— —	$\frac{1757}{807}$ 118 ,,	—	?
B	$\frac{1205}{764}$ 58 ,,	$\frac{1430}{1205}$ 19 ,,	$\frac{1430}{764}$ 87 ,,	38 ,,	6
H	$\frac{810}{505}$ 60 ,,	$\frac{983}{810}$ 21 ,,	$\frac{983}{505}$ 95 ,,	40 ,,	3
F	$\frac{1465}{792}$ 85 ,,	$\frac{1704}{1465}$ 16 ,,	$\frac{1704}{792}$ 115 ,,	45 ,,	1
M	$\frac{886}{728}$ 22 ,,	$\frac{1155}{886}$ 30 ,,	$\frac{1155}{728}$ 59 ,,	26 ,,	9
Total for 11 subjects	$\frac{11615}{7970}$ 45·7 °/ₒ	$\frac{13033}{11615}$ 12·2 °/ₒ	$\frac{13033}{7970}$ 63·5 °/ₒ	28·9 °/ₒ	

as an alternative, these quantities were expressed in percentage increase over the second five minutes' work of the *first experiment.* This is of course open to the criticism, that should a boy have done very badly, or exceedingly well in his first experiment, the whole of his subsequent results would be unreliable. But the choice of the *second* five minutes' work seems to have avoided this danger, since the results arrived at, on this method, agree very well with those derived from Table V, as may be seen in Table VI. Probably the fact that after the first few minutes, work is usually much more regular and steady, is the explanation, but certainly since two so widely different methods yield such similar results, we may regard the conclusions which we derive from them as possessing additional weight.

TABLE VI.

Comparison of Improvability by the two methods.

Position	1st	2nd
1	*F*	*F*
2	*L*?	*L*
3	*H*	*D*
4	*D*	*H*
5	*G*	*B*
6	*B*	*G*
7	*S*	*S*
8	*W*	*Y*
9	*M*	*W*
10	*Y*	*M*
11	*C*	*C*
12	*P*	*P*

We have now the choice of several methods (A, B, C, D, in Table VII) of arranging the subjects according to their improvability.

TABLE VII.

Position	A	B	C	D	Average
1	*F*	*F*	*F*	*L*	*F*
2	*L*?	*L*	*L*	*F*	*L*
3	*H*	*D*	*D*	*H*	*H*
4	*D*	*H*	*H*	*G*	*D*
5	*G*	*B*	*G*	*B*	*G*
6	*B*	*G*	*B*	*D*	*B*
7	*S*	*S*	*S*	*S*	*S*
8	*W*	*Y*	*Y*	*W*	*W*
9	*M*	*W*	*W*	*M*	*Y*
10	*Y*	*M*	*M*	*Y*	*M*
11	*C*	*C*	*P*	*C*	*C*
12	*P*	*P*	*C*	*P*	*P*

A. We may adopt the classification according to the average of α and β in Table V.

B. We may take the average of the percentage given by the second method as a basis.

C. Since the whole second series may possibly be regarded as abnormal, owing to the strain of preparation for school examinations (as mentioned previously), and the last experiment of the research may show undue end-spurt, we may ignore these four results altogether and draw up a list depending solely upon the remaining five tests.

D. We may take only the total improvement shown by the pauseless experiment of the third series over that of the first series. This is given in the $\gamma\frac{\text{III}}{\text{I}}$ column in Table V.

But as a matter of fact under all these circumstances, the relative positions are fairly well maintained, as will be seen from Table VII, and in dealing with the results we shall adopt the average of the four columns of this table.

Examining this and the foregoing results, we notice in the first place that in the final experiment (*A* 0 of the third series) all the subjects, with the exception of *P*, *W* and *Y*, made a noticeable spurt (probably owing to the nearness of the Easter vacation, which began the following morning). In the cases of *C* and *M* the result is particularly striking (see last column of Table V). In both of these instances there was in all probability unusual fatigue developed during the second series, their positions in the $\beta\frac{\text{III}}{\text{II}}$ column of Table V being particularly high as compared with those which they occupy in the other columns. Then again, we notice that whereas *S*, *D* and *F*, are at the top of the $\alpha\frac{\text{II}}{\text{I}}$ column in Table V, in the second column they occupy 10th, 9th and 7th places respectively. This may be due to either or all of the following causes:—

(*a*) a relative lack of will-tension in the third series,

(*b*) in the cases of *D* and *F*, to the very great amount of work done, or

(*c*) to an enormous initial improvement which was not maintained.

If we now compare these results with those given in Table III, for absolute amount of work done, we find that of the good workers *F*, *L* and *D* show also a high improvability, while *H* also stands very high, notwithstanding the comparatively small amount of work done.

On the other hand *C* shows both a low output of work, and low improvability.

A comparison of these two tables raises a number of interesting points which it would be well to consider subject by subject:

P (a highly neurotic and restless individual) begins with an enormous amount of work and steadily falls off, probably as a result of excessive fatiguability, and possibly owing to lack of will-tension in the later stages.

G takes a good position for improvability, probably because he starts with a rather low amount of work as compared with his later efforts. This may mean relative absence of will-tension at the outset, although it is of course possibly quite normal, but he does not display a similar characteristic in the multiplication tests.

C, as we have already noticed, makes a most marked spurt in the pauseless experiment of the third series, but he also seems to show evidence of considerable fatigue in the second series.

S falls off greatly in improvability in the β column of Table V, a result which seems to argue special exemption from fatigue in the second series (particularly when we compare this with his position in the α column) rather than any lack of spurt or will-tension in the third series, since generally speaking his improvability remains fairly high.

D closely resembles *S* in this respect.

L, as we have already noticed, takes a high position both as regards output of work and improvability.

H, although his absolute amount of work done is small, shows high improvability, a result which is in full agreement with his general school work.

F takes like *L* a high position in both cases.

M shows marked improvement in the β column of Table V, which as in the case of *C*, seems to indicate special fatigue in the second series, rather than any excessive effort in the third.

We thus have for absolute amount of work done, and improvability in addition, the following classification.

	1	2	3	4	5	6	7	8	9	10	11	12
Absolute work ...	*F*	*L*	*D*	*B*	*Y*	*W*	*P*	*G*	*M*	*S*	*H*	*C*
Improvability ...	*F*	*L*	*H*	*D*	*G*	*B*	*S*	*W*	*Y*	*M*	*C*	*P*

From this, and the remarks above, it will be seen that in a number of cases there is an evident relation between output of work and improvability, but at the same time no striking general relation exists. This is borne out by the coefficient of correlation calculated on Spearman's method (*vide supra*), which is ·37.

We now turn to the multiplication tests, the results of which were treated in a similar manner. Here again the two methods of determining improvability were found to agree very well, but lack of space forbids a detailed consideration of the data. Summarising briefly therefore, and taking the total from Table IV, we have for absolute amount of work done, and improvability in multiplication, the following classification:

	1	2	3	4	5	6	7	8	9	10	11	12
Absolute work ...	*L*	*F*	*D*	*B*	*Y*	*M*	*C*	*G*	*P*	*H*	*S*	*W*
Improvability ...	*B*	*S*	*C*	*F*	*H*	*W*	*G*	*P*	*Y*	*D*	*L*	*M*

A glance is sufficient to show that we have no correlation here, and this is confirmed by the coefficient of correlation which is as low as ·22.

Another question of interest, which may be worth investigation is how far improvability in the easier task is correlated with improvability at the more difficult multiplication. It might be expected that those who showed the greatest improvability in the one case would do so in the other, but this is by no means what occurs, for classifying as before we have:

Improvability in	1	2	3	4	5	6	7	8	9	10	11	12
Addition............	*F*	*L*	*H*	*D*	*G*	*B*	*S*	*W*	*Y*	*M*	*C*	*P*
Multiplication ...	*B*	*S*	*C*	*F*	*H*	*W*	*G*	*P*	*Y*	*D*	*L*	*M*

The coefficient of correlation, determined on Spearman's method, is found to be – ·007, a result which speaks for itself.

Before passing on, a comparison of the rates of improvability in the two cases may be made, since this is a question with which we have not dealt thus far. For this purpose the average output of work of all the subjects in each experiment is taken, and improvability calculated on the second of the methods described above. The results are shown below:

Addition	*A* 0	*A* 10	*A* 20	*A* 0	*A* 10	*A* 20	*A* 20	*A* 10	*A* 0
Experiments in Time order	—	23 %	41 %	47 %	58 %	54 %	64 %	66 %	67 %

Multiplication	*M* 0	*M* 10	*M* 20	*M* 0	*M* 10	*M* 20	*M* 20	*M* 10	*M* 0
Experiments in Time order	—	109·5 %	160 %	178 %	218 %	239 %	249 %	250 %	243 %

It will be at once seen that improvability is beyond all question far greater with the harder than with the easier task.

We have then thus far arrived at five fairly definite conclusions regarding output of work and improvability in the two kinds of mental work chosen. These we may state as follows:

1. There is distinct correlation between the amount of addition and the amount of multiplication done in a specified time. That is, generally speaking, those whose output of work is great at addition, behave similarly towards multiplication.

2. There is some amount of correlation between amount of work done and improvability in addition. To a limited extent only, those who work best at addition improve the most, and *vice versa*.

3. There is no correlation between amount of multiplication done in a given time and improvability. So far from those who do most being most improvable, there is a tendency in the reverse direction.

4. There is no correlation between the improvability shown in addition and that shown in multiplication, such as is present between the absolute amounts of work done at the two kinds of work.

5. The relative improvability appears much greater with the harder than with the easier task.

(*d*) *Retentivity of Practice.*

This may be determined according to a method based upon a principle used by Lindley[1] which consists in utilising the gain per cent. which is the result of the more favourable pause, as a basis for the calculation of an expected improvement upon the following occasion upon which work is done. The percentage difference between this expected amount, and that actually obtained we may employ as a measure of retentiveness. For example, subject *B* in the first series (addition) did 445 additions before the more favourable (10′) pause, and 520 after it. The increase is here equal to 16·8 °/₀ which gives an expected number of additions in 10 minutes upon the next occasion equal to 116·8 °/₀ of 520 = 607. He actually did 564, so that the loss was equivalent to 7 °/₀. Obviously the greater the retentivity of practice the smaller will be the observed loss. This method is by no means fully satisfactory, the dependence upon an expected quantity of work being its weakest point, but its claims are quite as strong as any of those which have been devised up to the present, and as it best meets the requirements of the present investigation it has been employed here. (See Table VIII.)

---

[1] *Psych. Arbeiten*, Bd. III. S. 482.

The difference between the second five minutes' work after the most favourable pause, and the second five minutes' work of the following experiment has also been suggested as a measure of retentivity of practice, but apparently the shortness of the experiments, and the varying intervals which occurred must have affected the results which were obtained upon these lines, since the large number of contradictory values which were yielded rendered its employment in the present case of no utility whatever. As a matter of fact, the whole question needs careful examination, since practice is so complex a phenomenon, that it is doubtful whether any method yet employed is fully adequate for the determination of completely reliable values. Certainly any method which is capable of giving at the same time positive and negative quantities as measures of retentiveness approaches dangerously near yielding its own *reductio ad absurdum.* On this account nothing more can be claimed for the results yielded than that they afford a basis for a rough comparison of the degrees of retentivity of practice displayed by the various subjects.

As has already been stated, no regular order of experiments was observed in the carrying out of the research, in order that the effect of intervals of varying length upon retentivity might be determined. The subsequent calculation of the results has shown that this was inadvisable, since while we have values for different intervals for both tests, we are thrown back upon the dangerous expedient of using only one value for each of these intervals, and have therefore no means of checking or confirming our results. A comparison of the tables of data obtained shows that the practice loss resulting from a varying interval varies with different individuals, so that while in a few cases a similar result occurs throughout, this is by no means general. Thus we have:

Interval	High	Indifferent	Low
1 day	*H B S M*	*D W C (G)*?	*P F Y L*
7 days.........	*H W C G B*	*S F Y*	*M D L P*

Taking both these positions and the percentage values in the tables into consideration we may assign to the various subjects the following positions according to retentiveness in the addition tests:

1	2	3	4	5	6	7	8	9	10	11	12
H	*B*	*W*	*C*	*S*	*G*	*M*	*D*	*F*	*Y*	*L*	*P*

When we turn to the multiplication tests, the results are found to be more satisfactory. The method employed was the same as that used in the case of the addition, and of course the same general criticism holds. We have however here, one interval of twenty-four hours (Series 1) with two of practically a week (Series 2 and 3), and in Table VIII are shown the figures obtained for the first of these.

TABLE VIII. *Multiplication.*

Interval 1 *day.* *Series* 1.

Subject	Before p.	After p.	°/₀ Impr.	Expected	Actual	Difference	°/₀ Diff.
P	190	289	52·1	433	293	140	32
W	120	165	37·5	226	177	49	21
G	145	188	29·6	242	217	25	10
C	158	185	17·08	216	178	38	17
S	131	173	32·06	228	145	83	36
Y	153	203	32·6	268	233	35	13
D	215	239	11·1	265	262	3	1
L	239	309	29·3	398	317	81	20
B	162	224	38·2	309	241	68	22
H	167	190	13·7	216	209	7	3
F	199	273	37·2	374	260	114	30
M	184	226	22·8	277	261	16	6

Here again, however, a comparison of the three sets of results revealed a somewhat marked irregularity, even when the interval was practically the same. This may, however, be due to some extent to the extra strain of the period during which the second series was carried out. According to the figures determined for the three series, we may classify the subjects as follows:

Interval	High	Indifferent	Low
1 day (1st Series)......	*D H M G*	*Y C W L B*	*F P S*
7 days (2nd Series) ...	*G D C*	*B S H Y F*	*L M W P*
7 days (3rd Series) ...	*B F G*	*C H D W*	*M L Y S P*

Notwithstanding the fact that the results are by no means regular, a number of interesting facts are noticeable. Thus *P* and *L*, as in the addition experiments, are feeble throughout. *D* and *M* also show the same tendency as in addition, the definite fall with the longer interval being especially marked in the case of the latter.

G is here consistently good throughout.

S is much higher in the second series than his general work would warrant, owing to his relatively small increase in that series after the more favourable pause. Otherwise his retentivity is poor.

Y steadily falls off and is apparently less retentive with longer intervals than the majority are. This seems also true as we have noticed of *D* and *M*.

F and *B* show marked superiority with the longer interval.

It will at once be seen that assigning any position for general retentivity must be here a matter of great difficulty, but taking everything into consideration the following has been adopted:

1	2	3	4	5	6	7	8	9	10	11	12
G	*D*	*H*	*C*	*B*	*F*	*M*	*W*	*Y*	*L*	*S*	*P*

The question now arises as to how far retentivity of practice in the addition tests is correlated with that observed in multiplication. If we write the two classifications down together we see that although there appears to be some resemblance between them, there is little of the nature of a general correlation. In fact this is clearly shown by the coefficient which is yielded by calculation, and which is as low as ·33, a figure which is of very slight value.

	1	2	3	4	5	6	7	8	9	10	11	12
A	*H*	*B*	*W*	*C*	*S*	*G*	*M*	*D*	*F*	*Y*	*L*	*P*
M_3	*G*	*D*	*H*	*C*	*B*	*F*	*M*	*W*	*Y*	*L*	*S*	*P*

Before passing on it might be mentioned that the author recognises that this part of the subject is one which requires much attention if genuinely reliable results are to be obtained, and it is with considerable reluctance that these figures for retentivity of practice are published.

Not only are the methods of calculation by no means beyond question, but retentivity of practice is itself an altogether different matter from retentiveness, and it is here that error is likely to arise unless the two terms are carefully distinguished. The figures, such as they are, refer merely to retentivity of practice, and nothing more. To argue anything from them regarding retentiveness, seems to the author an altogether fallacious proceeding. Until we can arrive at some simple method of estimating retentiveness in such tests as these, without having to fall back upon retentivity of practice (and that calculated on a method that it must be admitted hardly carries conviction), this side of the subject must always be a matter of uncertainty.

(e) *Fatiguability.*

If we assume that the output of work in any series of experiments, such as those we are at present dealing with, is the best possible after the more favourable pause, then it has been suggested we may with very fair ground, take the percentage increase after that pause as a basis for estimating fatiguability. For under the same circumstances, a similar increase might have been expected during the second half of the corresponding pauseless experiment, but for the loss due to fatigue. If therefore we calculate this expected amount, and compare it with the amount actually done, the percentage difference may be taken as an approximate measure of fatiguability. This method has been used by Lindley[1], but is of course open to the objection that it is hardly sound from a mathematical point of view. Without a rigid application of the theory of probabilities such results as are yielded, can be nothing more than probable, resting as they do upon an expected amount of work. It has, however, worked very satisfactorily, and the results given have been arrived at on this method. In an appendix to this paper the author suggests a much simpler scheme for the estimation of fatigue, and one which at the same time is not open to the above criticism. It was not employed in this research however, and need not be introduced at this point. As a further illustration of the older method, let us take an example:

Subject *L* in the first series obtained in addition after his more favourable pause an increase of 26·2 °/₀. During the first half of the pauseless experiment he did 370 additions. But for the unfavourable conditions of the continuous work therefore we expect he would have done 126·2 °/₀ of 370, 466 additions. As a matter of fact the actual amount done was 437 which is equivalent to a loss of 7 °/₀.

In Table IX are given the addition results for each subject in the third series calculated along these lines, but as might have been expected these results are by no means uniform throughout, although some subjects display a like tendency in each series. Doubtless the most satisfactory summary is that given in Table X, where it will be noticed that in many cases some definite tendency is observable, even amid the variations. For instance it is clear that *P* is highly fatiguable and becomes more noticeably so as work progresses. *W* again, while only slightly fatiguable, seems to become even less so as work progresses. *G*, who yields only two results, shows up very much worse in the last

---

[1] *Psych. Arbeit.* Bd. III. S. 482.

TABLE IX.

Addition. 3rd series.

Subject	*M F P* % Gain	1st half 0 pause expected	Expected	2nd half Actual	Difference	%
P	74	844	598	360	238	40
W	2·4	560	573	562	11	2
G	14	601	685	582	103	15
C	-0·2	508	507	524	17	3
S	6·7	541	577	489	88	18
Y	12·7	604	681	613	68	10
D	18·9	722	858	751	107	13
L	12·6	864	975	893	82	8
B	12·4	784	825	696	129	15
H	-1·4	499	492	484	8	2
F	11·05	845	938	859	79	8
M	18·1	576	680	579	101	15

TABLE X.

Fatiguability. Addition.

Series	Low	Indifferent	High
I	*F B D M*	*C Y*	*W P S L H*
II	*B D C*	*W G F H*	*P M Y S*
III	*H W C*	*F L Y D S*	*G B M P*
Average......	*C F D B W*	*H L M Y*	*G S P*

series. *C* is very slightly fatiguable, and improves as time elapses. Like *W*, *S* is generally highly fatiguable. *D*, although only slightly fatiguable, shows like *P* and *G* a falling off as the experiments proceeded. (Compare in this respect also *B* and *M*.) *H* shows a remarkable decrease in fatiguability as time elapses, and moves from the lowest position in the first series to first place in the third.

The multiplication results were treated along the same lines as are shown in Table IX., and quite as great a diversity was noticed here as was seen in the case of the addition. In passing we may notice the following interesting facts :—

The fatiguability of *P* was less marked here than was that of *W* and *S* who were highly fatiguable throughout, but whereas the latter displayed the same tendency as in addition, *W* appeared much more fatiguable with the more difficult task than with the easier one.

TABLE XI.

Fatiguability. Multiplication.

Series	Low	Indifferent	High
I	*H D G*	*C P B L F*	*M Y W S*
II	*M Y H*	*D F C B G*	*P W S*
III	*B Y G C*	*D M F H*	*P L W S*
Average......	*B D G C H*	*M Y F P*	*L W S*

G and *C* on the other hand were slightly fatiguable throughout, *C* giving a similar result to that obtained at addition, while *G* showed a rather different tendency. *Y* apparently became less fatiguable as time elapsed. *D* was slightly fatiguable throughout, and rather more definitely so here than in the addition tests. *L* was distinctly highly fatiguable. *B* showed a marvellous decrease in fatiguability as time went on, his average coming out smallest among the twelve subjects. *H* showed an increase rather than a decrease of fatiguability with progress of time, so that, like *F*, he reversed his addition result. *M* came out distinctly better with multiplication than he did with addition.

The first point of importance which arises from our consideration of these data, is that increase of difficulty of mental work by no means affects all individuals in the same way, and examination of Tables X and XI, shows that practically no correlation exists. In fact the coefficient is as low as ·2.

In the next place we may attempt to compare the degree of fatiguability exhibited at addition with that shown at multiplication, using the averages of the work-totals of the twelve subjects just as was done for improvability. By utilising the method already employed for the determination of individual fatiguability, we arrive at the following results:

Addition.

Series	More Fav. Pause	Incr. %	Expected	Actual	Difference %
I	10′	11·6 %	887	889	0·6
II	20′	8·2 „	585	544	−7
III	10′	10·7 „	688	616	−9·8

Multiplication.

Series	More Fav. Pause	Incr. °/₀	Expected	Actual	Difference °/₀
I	10′	28 °/₀	121	93	−23
II	20′	4 ,,	272	261	−4
III	10′	10 ,,	346	330	−4·6

From these figures it appears that whereas at the outset the multiplication was by far the more fatiguing, as work progressed (Series 2 and 3) the more mechanical addition greatly increased in fatiguing capacity, whereas the harder task very greatly decreased. It seems therefore quite safe to assume, that while at the outset fatiguability is greater with the harder work, yet apparently as time goes on (*i.e.* as we pass from series to series) the more difficult task becomes less fatiguing than the easier *but more mechanical* one.

III. THE RELATIONS BETWEEN IMPROVABILITY, RETENTIVITY OF PRACTICE, FATIGUABILITY AND OUTPUT OF WORK.

We have now to consider what relations are discernible between the four phenomena, improvability, retentivity of practice, fatiguability, and output of work so far as we have been able to estimate their values. Table XII summarises our general conclusions with regard to the easier task, and it is with that we shall first deal.

TABLE XII.

Subject	Output of work	Improvability	Retentivity	Fatiguability
P	Indifferent	Low	Low	High
W	Indifferent	Low	High	Low
G	Indifferent	High	Indifferent	High
C	Low	Low	High	Low
S	Low	Indifferent	Fairly high	High
Y	High	Low	Low	High
D	High	High	Indifferent	Low
L	High	High	Low	Indifferent
B	High	Indifferent	High	Low
H	Low	High	High	Indifferent
F	High	High	Low	Low
M	Low	Low	Indifferent	Fairly high

As we have already noticed, Kraepelin and his school hold that high improvability is most frequently combined with low retentivity of practice on the one hand, and invariably with high fatiguability on the other. Such an assertion must strike anyone engaged in educational work as rather too sweeping, particularly when the small number of cases investigated is considered.

It will be seen from Table XII that ignoring output of work, which hardly enters into the question, the twelve subjects may be classified along the following lines:

(i) Improvability and fatiguability.
(ii) Improvability and retentivity of practice.
(iii) Retentivity of practice and fatiguability.

(1) With regard to the first of these we find that in four cases, viz. *W*, *G*, *C* and *B*, fatiguability and improvability are alike, that is to say, high fatiguability is combined with a high improvability and *vice versa*. On the other hand in the remaining eight cases the two are unlike, high improvability being found with low fatiguability or the reverse.

(2) If we consider the relation of improvability to retentiveness, it will be noticed that in five cases, viz. *P*, *S*, *Y*, *H* and *M*, distinctly the same tendency is displayed, while in the remaining seven, the reverse appears. That is to say, about half of the subjects tested showed high improvability combined with high retentivity of practice, or low improvability combined with low retentivity, instead of there being, as we should expect from the investigations of Kraepelin and his pupils, a considerable balance in favour of the inverse relationship.

(3) When we come to consider the question of the relation between retentivity of practice and fatiguability, we find no less than eight subjects who manifest an unlike tendency in the two cases. The remaining four, *S*, *D*, *L* and *F*, exhibit a similarity of fatiguability to retentivity of practice both being high or low, but even these four subjects are not entirely beyond question. We have therefore here some indication of a possible inverse relationship between the two, upon which we shall have something to say at a later stage.

Our conclusions under heads (1) and (2) therefore, point to the fact that although undoubtedly improvability and retentivity of practice are closely related, we are not in a position to affirm any general statement regarding that relationship. Kraepelin and his pupils have certainly examined many cases in which the two differed considerably in degree, and their position is indicated by Specht in his *Ueber klinische Ermüdungsmessungen*[1], where he says, "Lindley[2] found that his Vp. C possessed inferior improvability but great retentivity of practice, whilst in Vp. A the reverse was the case. This cohesion does not always exist, but certainly from numerous later tests it has appeared that great improvability occurs with trifling retentivity on the one hand, and with great fatiguability on the other, a relation which points to a great fundamental principle underlying these characteristics." If this be so, then it is remarkable that the subjects in the present research should not have afforded more definite evidence in support of the theory. Certainly we are left no alternative but to withhold adherence from such an assertion until more evidence is forthcoming.

---

[1] *Arch. f. d. ges. Psychol.* 1904, Bd. III. S. 245.

[2] *Op. cit.*

Output of work has obviously so little relation to either of the other three factors given in the previous table, that its consideration is hardly worth entering into. In fact the present research fully bears out Lindley's contention that there is no fixed relation between the height of the '*Arbeitsleistung*' and the characteristics of the fatiguability, improvability, and retentivity of practice displayed. We now turn therefore to the multiplication results and treating them in the same way we will endeavour to discover to what extent difference of task modifies the relations already noticed in the case of the easier work, between improvability, retentivity, fatiguability and output of work. Table XIII summarises our general conclusions thus far, and here again a glance is sufficient to show that no general relation of the kind assumed by Kraepelin is discernible.

TABLE XIII.

Subject	Output of work	Improvability	Retentivity	Fatiguability
P	Low	Low	Low	High
W	Low	Indifferent	Low	High
G	Low	Indifferent	High	Low
C	Indifferent	High	High	Low
S	Low	High	Low	High
Y	High	Low	Low	Indifferent
D	High	Low	High	Low
L	High	Low	Low	High
B	High	High	High	Low
H	Low	High	High	Low
F	High	High	Indifferent	High
M	Indifferent	Low	Indifferent	Indifferent

Detailed examination reveals the following interesting facts:

(1) With regard to improvability and fatiguability, in six cases (*G, S, Y, D, F, M*) the two are alike, that is to say, high improvability

is combined with high fatiguability and *vice versa*, whereas in the remaining six cases (*P*, *W*, *C*, *L*, *B*, *H*) the reverse is the case.

(2) An even more striking classification is arrived at when we consider retentivity of practice in relation to improvability. In four cases (*G*, *S*, *D*, *F*) the two are reversed, and these are not all entirely beyond question, while on the other hand the remaining eight subjects display a distinctly similar tendency in the two cases.

(3) There is a most remarkable inverse correlation between retentivity of practice and fatiguability. We noticed a tendency in this direction when dealing with the easier work. Here it is strikingly evident, Table XIII making the fact sufficiently obvious. There can be no doubt whatever that a relation of this type does exist. Work in schools has long pointed to the fact that a low retentivity of practice is commonly allied with a high fatiguability, and a high retentivity of practice with a low fatiguability, so that after all, such a result can be by no means unexpected. At least its attainment in this research goes far to testify to the reliability of the work done by the subjects, and the genuineness of the doubt which the results as a whole cast upon the theory already quoted as to the nature of the relationship which exists between improvability, fatiguability and retentivity of practice. In fact the whole of the work already described simply points to a fundamental principle of supreme importance for all work of this type, namely that in psychological research we are dealing with phenomena far more complex than those which constitute the subject matter of the physical sciences, and that consequently before any generalisations can be made, it is absolutely necessary to widen the field of investigation to a very considerable degree. The utter lack of anything approaching unanimity in the majority of the various relationships we have thus far investigated, leaves us no alternative but to recognise the extreme complexity of the problem and, until our data are much more complete, to refrain from basing conclusions upon evidence which is far too meagre for so involved a subject as mental work.

IV. THE SECOND RESEARCH.

The purpose of the second investigation to which reference has already been made, was obviously to determine how far the conclusions arrived at in the previous research were supported by the results obtained when the character of the task was changed. Undoubtedly it would have been of great advantage to have been able to utilise the same subjects again, but the exigencies of school work and time-tables, would not admit of this being done. As we have already seen the tests consisted throughout the research of two kinds of mental work, namely mental multiplication of two figures (M_2) as the easier task, and mental multiplication of four figures (M_4) as the harder exercise.

(*a*) *Incitation and the more favourable pause.*

As will be seen from Table XIV, the results were arranged on a plan identical with that adopted previously, and a careful consideration of the percentages revealed a number of facts of the same character as those noticed in the earlier investigation. In the first place the shorter rest pause (10′) was seen to be by far the more favourable of the two, and on the whole more markedly so with the harder task. To this rule, however, *F* and *G* are notable exceptions, the latter being highly fatiguable, and showing throughout a decided preference in favour of the longer pause with the harder task, and the former exhibiting a curious tendency with which we shall have to deal immediately. In the next place, in quite a number of cases, the pauseless experiment yields the most favourable result. These are,

TABLE XIV. (M_3.) *Third Series.*

Sub-ject	1st half	2nd half	$M_2 0$	Gain %	1st half	2nd half	$M_2 10$	Gain %	1st half	2nd half	$M_2 20$	Gain %
A	411	375	786	-8·7	396	382	778	-3·5	392	402	794	2·5
B	424	464	888	9·4	489	501	990	2·5	475	479	945	0·8
C	859	843	1702	-1·8	881	933	1814	5·9	897	914	1811	1·8
D	412	391	803	-5·1	429	516	945	20·2	424	437	861	3·1
E	577	551	1128	-4·5	517	551	1068	6·5	540	547	1087	1·3
F	501	537	1038	7·1	539	566	1105	5·1	543	527	1070	-2·9
G	1010	1031	2041	2·1	1040	1120	2160	7·7	1042	1101	2143	5·6
H	625	600	1225	-4·0	640	705	1345	10·2	591	623	1214	5·4
I	754	773	1527	2·5	767	848	1615	10·6	801	845	1645	5·4
J	717	717	1434	0·0	729	812	1541	11·4	712	747	1459	4·9
K	466	461	927	-1·0	489	494	983	1·1	486	506	992	4·1
L	562	528	1090	-6·0	562	582	1144	3·6	551	547	1098	-0·7

	1st Series	2nd Series	3rd Series
M_2.........	*A D F I J K*	*A E F*	*B F*
M_4.........	*B J K*	....	*B C E J L*

Such a state of things must strike one as very remarkable, especially such cases as *A*, *B*, *F*, and *J* in which the result cannot be regarded in any sense as accidental. It is interesting, therefore, to notice that both *B* and *F* in their introspective notes, complained that the pause 'spoilt' or 'hindered' their work. The latter, in fact, upon one occasion definitely stated that he was barely "getting into the swing" when he was told to stop, and he had "to make a fresh start" after the pause. There can be little doubt therefore that incitation is very largely responsible for what we find, although it is curious that the favourability of the pauseless work-period is rather more marked with the easier task than with the harder one, for even though the multiplication of two figures and the multiplication of four figures are similar in character, there can be no question as to the difference in difficulty.

(*b*) *Absolute Work.*

The absolute work done (irrespective of work after pauses) was tabulated just as before and perhaps the most noteworthy fact demonstrated, was the much greater degree of regularity which these twelve subjects showed as compared with the previous twelve. Thus, if we examine the (M_1) results, we find the subjects may be classified as follows:

TABLE XV.

Series	Good	Indifferent	Bad
I	*G C I J*	*H L E K B*	*F D A*
II	*G C I J*	*H L E K F*	*B A D*
III	*G C I J*	*H L E F*	*K B D A*
Total	*G C I J*	*H L E K F*	*B A D*

Thus except for an unimportant change or two in the cases of *K*, *F* and *B* the same order is maintained throughout. When we turn to the harder task we find not only a similar state of things, but considerable co-relation between the two cases. Classifying again, just as we did for the easier work in Table XV we obtain:

TABLE XVI.

Series	Good	Indifferent	Bad
I II III	*C G J I* *C G J H* *C G J H*	*H K B L* *I K B L* *I E K B*	*E F D A* *E F D A* *L D F A*
Total	*C G J I*	*H K E B*	*L D F A*

(c) *Improvability.*

Improvability was dealt with upon the same lines as in the earlier research, with practically identical results.

Considering the (M_2) work first, we find that all the good workers take a high place for improvability, and this is particularly marked in the cases of *G*, *I* and *J*. This seems hardly what we should expect, since one would imagine that the poorer workers would show most improvement, rather than those who started out with a relatively large amount. In any case it is a striking testimony to the reliability of the subjects, and the thoroughly genuine effort which they made to work conscientiously. On the other hand *D* and *B* come out poor in both cases, while *A* notwithstanding his low output of work shows on the whole fairly high improvability. So that we may classify the work at the easier task as follows:

	1	2	3	4	5	6	7	8	9	10	11	12
Absolute work ...	*G*	*C*	*I*	*J*	*H*	*L*	*E*	*K*	*F*	*B*	*A*	*D*
Improvability ...	*I*	*J*	*G*	*F*	*A*	*C*	*L*	*E*	*B*	*H*	*K*	*D*

Here again, it will be seen that there seems to be a distinct relationship in some cases between output of work and improvability, although but slight correlation exists between the two series. The coefficient of correlation (·33) which is yielded on Spearman's method quite confirms this, and closely resembles that obtained in the earlier research.

When we examine the work done in the harder tests, we meet with a result strikingly similar to that previously described. For here indeed, not only is there no direct correlation, but in some cases a tendency towards an inverse relationship is apparently manifested. Classifying as before we obtain:

	1	2	3	4	5	6	7	8	9	10	11	12
Absolute work ...	*C*	*G*	*J*	*I*	*H*	*K*	*E*	*B*	*L*	*D*	*F*	*A*
Improvability ...	*A*	*C*	*J*	*K*	*B*	*D*	*E*	*G*	*H*	*I*	*F*	*L*

The positions of *G*, *I*, *H*, *D* and *A* show a considerable degree of inversion, but at the same time *C*, *J*, *K*, *E* show an equally direct relation, so that altogether the coefficient of correlation which works out at ·16 may be regarded as very well representing the degree of correlation which exists.

The parallelism of result in the two researches by no means ends here, for when we turn to a comparison of improvability at the easier task with that shown at the harder work we find an equally remarkable disagreement with that previously observed. This is obvious from the following classification:

	1	2	3	4	5	6	7	8	9	10	11	12
Improvability (M_2)...	*I*	*J*	*G*	*F*	*A*	*C*	*L*	*E*	*B*	*H*	*K*	*D*
Improvability (M_4)...	*A*	*C*	*J*	*K*	*B*	*D*	*E*	*G*	*H*	*I*	*F*	*L*

Here the coefficient of correlation is found to be −·13 a result which quite confirms our earlier conclusion that subjects whose improvability is highest at the one task, by no means exhibit the same characteristic when the degree of difficulty of the work is changed.

It will be remembered that in the earlier research it was found that the improvability shown at the harder task was beyond question greater than that exhibited at the easier work. It might be of interest therefore to examine this question before passing on. In Table XVII are shown the percentages obtained when the average of the work done by the twelve subjects in the second five minutes of each experiment is expressed as a percentage increase over that done in the second five minutes of the first experiment.

TABLE XVII.

	1st Series			2nd Series			3rd Series		
	0	10	20	0	10	20	0	10	20
M_2...	—	24·3	36·4	40·6	48·3	46·7	52·9	56·8	55·7
M_4...	—	15·4	32·2	33·1	44·1	47·7	60·0	66·9	67·8

The result is curious, and although it points to the same conclusion as was arrived at earlier, it is by no means of exactly the same character. It would appear that improvability is rather less at first at the harder task than it is at the easier one, but that in the later stages it becomes somewhat greater. If we allow for the facts that the mental multiplication of four figures is a matter of considerable difficulty, and therefore one in which the effect of incitation is of necessity well marked, and also that the M_4 work bears a somewhat different relation to the M_1, from that which the A work bears to the M_3, we may assert here, as in the previous experiments, that improvability is greater at the harder than it is at the easier work. At the same time, however, incitation may materially modify the result obtained.

(*d*) *Retentivity of Practice.*

The arranging of the various experiments so that the intervals throughout were the same, enabled the author to utilise both the methods of estimation referred to earlier in these pages. This was a matter of considerable advantage as it afforded opportunity for a careful comparison of the two methods, neither of which can be said to be fully satisfactory, and of the results attained. Of the two, the difference method seems to be much the more satisfactory, on account of its simplicity, and also since it avoids the objectionable calculation of an expected amount of work. The difference between the work done at the most reliable period (2nd 5′) when the subject is at his best (after the most favourable pause) and that done at the corresponding period of the following experiment, seems undoubtedly a legitimate measure of his retentivity of practice, but one cannot help feeling that the yielding of both positive and negative quantities for such a function is a great weakness. It was felt however, that since the two methods approach the question from somewhat different standpoints, the most satisfactory plan was to take them together, particularly when it is noticed that in no case are the positions of those who are markedly good, or markedly poor, affected by this procedure. We shall therefore adopt throughout, the positions arrived at in this way. These are given in Table XVIII, and it will be seen that although the variations from series to series are marked, and those which appear upon comparison of the two methods somewhat serious, yet there can be little doubt that *A*, *B*, *F* and *L* are highly retentive, while *J*, *D*, *G* and *K* are distinctly feebly so.

TABLE XVIII. (M_2.)

Series	1st Method			2nd Method		
	High	Indifferent	Low	High	Indifferent	Low
I	*B A F H*	*D K C*	*J G I E L*	*A B*	*F D H G J I*	*C K L E*
II	*L E A F*	*H K C B*	*D I G J*	*L H E F*	*A D I C B*	*K C J*
III	*B E L F A* (?)	*C G I K* (?)	*J H D*	*L E F A* (?)	*B C I G*	*J H D K* (?)
Average	*A B F L*	*E K C H I*	*G D J*	*A B F L*	*I H C E*	*D G K J*

A very similar result to the above was obtained with the M_4 data, so that we may now pass on to the investigation of the correlation existing between retentivity at the easier and the harder tasks. Classifying from the Tables and arranging the two classifications together we obtain:

	1	2	3	4	5	6	7	8	9	10	11	12
M_2 ...	*A*	*B*	*F*	*L*	*E*	*C*	*H*	*I*	*K*	*D*	*G*	*J*
M_4 ...	*B*	*E*	*C*	*D*	*H*	*I*	*L*	*G*	*F*	*K*	*A*	*J*

and it will be noticed that *B*, *H*, *I*, *K* and *J* occupy very similar positions in the two cases, but on the other hand *A*, *F* and *D* differ very greatly. It cannot therefore be asserted that any general relation exists between the two, a conclusion which is supported by the coefficient of correlation, which is found to be ·16.

Finally with a view to comparing the degree of retentivity of practice exhibited at the easier task with that present at the harder one, an average of the various tables was made which yielded the following percentages:

Average retentivity percentage for 12 subjects (M_2) 1st Method −8·0 °/o
" " " " " " 2nd Method −2·0 "
" " " " " (M_4) 1st Method −3·6 "
" " " " " " 2nd Method +1·5 "

So that there is apparently a somewhat greater degree of retentivity developed at the harder task than at the easier and more mechanical one. In passing, it may be mentioned that in the earlier research the varying intervals between the experiments so affected the results determined for retentivity of practice, that although there was a distinct indication of a lower degree of retentivity with the easier task, the figures obtained were of too doubtful a character to be worthy of particular notice.

(*e*) *Fatiguability.*

While in this research the author has adopted as before the method of finding a value for fatiguability by the method adopted by Lindley, he has also introduced a much simpler one based upon rather different lines. The results yielded are distinctly interesting since there is absolute agreement, not merely in the positions which are yielded for each subject for the two methods, but also to a very close approximation in the actual percentages yielded. A detailed description of the method is given in the Appendix to this paper.

The striking agreement which was observed between the two methods, rendered the classification of the subjects a simple matter, and the tables obtained require very little comment, except that in the M_2 experiments, while *F*, *A*, *I*, *B* are slightly fatiguable *L*, *G*, *H* and *D* are highly so, while in the (M_4) tests we have *J*, *B* and *C* slightly, and *G*, *D*, *H*, *A* highly fatiguable. We might, however, just examine how far there is any correlation present. Classifying in the ordinary way we have:

	1	2	3	4	5	6	7	8	9	10	11	12
M_2 ...	*F*	*A*	*I*	*B*	*E*	*K*	*J*	*C*	*L*	*G*	*H*	*D*
M_4 ...	*J*	*B*	*K*	*C*	*E*	*I*	*L*	*F*	*G*	*D*	*H*	*A*

It can at once be seen that no correlation whatever is apparent, and this is supported by the fact that the coefficient of difference is ·16.

Finally before leaving the question of fatiguability, we may briefly attempt to compare the degree of fatiguability which is exhibited at the two tasks. Taking as before the averages of the work totals of the twelve subjects, and utilising the method employed for the determination of individual fatiguability, we obtain the following percentages:

	1st Series	2nd Series	3rd Series
M_2	0·75	−4·7	−8·7
M_4	−0·9	−12·0	−0·1

As before there is a distinct indication that although the harder task appears to be more fatiguing, at first, yet eventually a change occurs and in the later stage the easier and more mechanical work begins to become more fatiguing. The increase in the latter case is particularly evident.

V. RELATIONS BETWEEN THE PHENOMENA.

We can now pass on to a consideration of the relations which exist between the four phenomena, output of work, improvability, retentivity of practice, and fatiguability, and in Table XIX the subjects are arranged as in Table XII, to show the results which have been arrived at for the easier task (M_2).

TABLE XIX. (M_2.)

Subject	Absolute work	Improvability	Retentivity	Fatiguability
A	Low	Fairly High	High	Low
B	Low	Low	High	Low
C	High	Indifferent	Indifferent	Fairly high
D	Low	Low	Low	High
E	Indifferent	Indifferent	Fairly High	Low
F	Indifferent	High	High	Low
G	High	High	Low	High
H	High	Low	Indifferent	High
I	High	High	Low	Low
J	High	High	Low	Indifferent
K	Indifferent	Low	Low	Indifferent
L	Indifferent	Indifferent	High	High

Classifying as before according to (1) improvability and fatiguability, (2) improvability and retentivity of practice, and (3) retentivity of practice and fatiguability, we find that the results quite bear out the conclusions arrived at in the first research. For we find:

(1) With regard to the relation between improvability and fatiguability, there are five cases in which the same tendency appears in the two (*B*, *C*, *E*, *G*, *K*), and seven in which they are reversed (*A*, *D*, *F*, *H*, *I*, *J*, *L*). This classification certainly somewhat favours inverse rather than direct correlation.

(2) When we turn to the question of improvability and retentivity of practice, we find that in seven cases there is a similarity of tendency (*A*, *C*, *D*, *E*, *F*, *H*, *K*) while in the remaining five (*B*, *G*, *I*, *J*, *L*) the two are reversed.

(3) Retentivity and fatiguability show a slightly less definite relationship than was apparent with the easier task of the earlier research. Four (possibly five) subjects display a similar tendency in the two (*I*, *J*, *K*, *L* and *C*) while seven exhibit a reverse tendency (*A*, *B*, *D*, *E*, *F*, *G*, *H*).

This is hardly as conclusive as one would have expected after the striking result arrived at with multiplication in the earlier research, but there does undoubtedly appear to be a close relationship between cases of high fatiguability and low retentivity and *vice versa.* Indeed, this is only what one would expect.

Output of work, as before, stands in no definite relation to either of the other factors in mental work, and need not therefore be considered.

Turning now to the difficult work, and treating the figures in the same manner, we may tabulate the subjects as shown in Table XX. It can at once be seen that the results are very similar to those already arrived at.

TABLE XX. (M_4.)

Subject	Absolute work	Improvability	Retentivity	Fatiguability
A	Low	High	Low	High
B	Indifferent	High	High	Low
C	High	High	High	Low
D	Low	Indifferent	Fairly high	High
E	Indifferent	Indifferent	High	Indifferent
F	Low	Low	Low	High
G	High	Indifferent	Indifferent	High
H	High	Low	Fairly high	High
I	High	Low	Indifferent	Indifferent
J	High	High	Low	Low
K	Indifferent	High	Low	Low
L	Indifferent	Low	Indifferent	Indifferent

Examining this table in somewhat greater detail, we notice the following relationships:

(1) As far as improvability and fatiguability are concerned, we find that four subjects (*A*, *E*, *I*, *L*) exhibit a similar tendency while the rest cannot be said to show any such relationship as Lindley and the other members of Kraepelin's school suggest. What correlation there is, is rather of an inverse character.

(2) When we consider improvability and retentivity of practice, we find that seven subjects show a similarity between the two (*B, C, D, F, G, I, L*), while five (*A, J, K, E, H*) give a different result. Again, therefore, we must regard the evidence as favouring the conclusion that improvability and retentivity are at least as often similar in character as otherwise.

(3) With regard to retentivity of practice, it will be noticed that low retentivity is combined with high fatiguability (or *vice versa*) in six (possibly eight) cases (*A*, *B*, *C*, *D* (?), *E*, *F*, *G*, *H* (?)) while in the remaining four cases the same tendency is displayed. This quite bears

out our former conclusion regarding the very close inverse relationship which exists between the two. One would have expected a somewhat more definite result in this case however, after comparison with that yielded in the earlier research, but the fact that the four subjects I, J, K and L are the exceptions in *both* the easy and the more difficult task, is rather striking, although there is of course no reason why they should occur together at the bottom of the list rather than in any other position. On the whole therefore we may regard the evidence of both researches as distinctly in favour of the assertion that high fatiguability most usually occurs in combination with low retentivity of practice, and *vice versa.*

VI. CONCLUSIONS.

We have thus considered the relative effects of practice and fatigue as manifested with easy and difficult tasks (of similar character) along similar lines, in the two researches; it may be noticed that there are almost identical results. These may be summarised as follows:

Treating the data *generally*, it has been found that:

I. The shorter rest-pause (10′) is (unexpectedly) much more generally favourable with the harder task than it is with the easier one.

II. The relative improvability appears much greater with the harder than with the easier task. (This is somewhat veiled in the earlier stages of the second research, probably by the high degree of incitation which is developed at the M_4 work.)

III. At the outset fatiguability is greater with the harder work, but apparently as time goes on the more difficult task becomes less fatiguing than the easier (more mechanical) one.

IV. It is at least probable that retentivity of practice is greater with the harder than with the easier task.

An examination of the *individual* data has shown that:

I. Those subjects whose absolute amount of work at the easier task was greatest, generally also did the greatest amount at the harder task. Their improvability, however, did not show a similar correspondence.

II. Those who did most work at the easier task, to a slight extent showed the highest improvability; but this was by no means the case with the more difficult task.

III. Taking everything into consideration, there is as much evidence in favour of similarity of degree of improvability and retentivity of practice in the same individual, whether mental work be easy or difficult, as there is in favour of high improvability being combined with low retentivity of practice or *vice versa*. This result is inconsistent with the general conclusions of Kraepelin and his school.

IV. In both researches, whether the task was easy or hard, high improvability occurred with low fatiguability, or low improvability with high fatiguability, in rather more cases than did the similarity of tendency in improvability and fatiguability, which Kraepelin and his pupils have found.

V. A very considerable inverse relationship was apparent throughout between retentivity of practice and fatiguability.

In conclusion it might be remarked that certain modifications of Kraepelin's '*Rechenhefte*' are advisable should they be employed for multiplication tests of the type described in these pages. In particular the figure 5 should be omitted, since its presence combined with that of an even number, leads the subject after a time to know that the figure to be written down must be a 0, and there can be no doubt that knowing this fact, one would inevitably write down the product without performing the mental operation which the other multiplications involve. Indeed in this and many other respects the whole investigation of which this is an account has proved highly suggestive in character, and has afforded a large number of data which will thoroughly repay further consideration. Thus the work curves would certainly repay analysis, fatiguability might be considered in reference to the pauseless series which succeed (instead of those which precede). Spurt and incitation again have been but barely looked at, but it is felt that the present paper has reached limits, transgression of which would render its perusal wearisome, and these questions must therefore be held over for the present. Sufficient has at least been said to convey an idea of the earnestness and reliability of the subjects, and their suitability for such experiments. With *their own teachers* such children form ideal subjects, and enter into the spirit of the experiments with a zest that is surprising even to one who has long known the 'human boy.' But it is exceedingly doubtful whether any stranger visiting a school purely for experimental purposes, can achieve results with the regularity and utility of those given in this paper. Even the older school children are exceedingly sensitive to variations of the normal routine, and even the slightest change has been found by the author to produce striking and otherwise unaccountable abnormalities.

APPENDIX A.

Introspective notes upon the physical factors entering into the preceding tests.

During the progress of the investigations described in the foregoing paper, the author made an attempt to discover by means of introspective notes by the subjects, how far such factors as eye-strain, muscle-strain, liability to distraction in the two cases, headache, etc., entered into the experiments. For this purpose the following method of procedure was adopted:—After six tests (three of each kind) the subjects tested in each research were requested to answer a series of questions upon these points, and add any other notes of their own regarding any features not covered by the questions, but which had struck them during the progress of the work. The outcome of this series of questions was a number of very interesting observations, which are given below.

In order to avoid going into too great detail, the answers to the questions will be taken together, point by point rather than separately. Thus summarised, the following results are obtained:

(*a*) In considerably over half of the cases, direct mention is made of a sense of *increasing fatigue* as work proceeded, and of these by far the greater number specify that this was more marked in the harder than in the easier tests. One subject naïvely remarked that whereas in the latter he experienced muscle fatigue, in the former his mind was fatigued. Some complain of a weariness and a desire to sleep, others of a worn-out feeling or of being muddled and not able to get on. K and L found the work (M_4) seemed to get more difficult, while H found his head feeling heavy, and an utter disgust setting in. Of course the bulk of these symptoms followed the pauseless tests.

(*b*) Very little *eye-strain* was noticed in the first research; only three subjects mentioned it at all, two after continuous addition, but one (P) upon several occasions and with both kinds of work complained of the presence of eye-strain. This is interesting in view of the very low improvability and high fatiguability of this subject. On the other hand, in the second research, in about half of the cases investigated in the M_3, and about one-fourth in the M_4 tests there is very definite reference to eye-strain, with accompanying dizziness and headache. This is a very much higher percentage and is in the author's opinion almost entirely due to the fact that the room in which the tests were carried out was much less satisfactorily lighted than the one used for the earlier investigation.

(c) With regard to *liability to distraction* the general consensus of opinion in the earlier research seems to indicate a greater liability in the case of the easier task, while on the other hand in the later investigation, the testimony of the subjects varied so greatly as to be of little value. Some subjects gave one the impression that such liability depended entirely upon the degree of weariness experienced, and that the very fact that their attention was voluntary rather than spontaneous, led them to recognise a greater liability to be readily distracted, as the result of a flagging will-tension, in the case of the harder task.

(d) There was practically complete agreement throughout that greater *concentration of attention* was required at the harder task, the easier work in each case being performed mechanically. On this point subject *I* stated 'I have to think about the four figure multiplication, but I do not have to do so with the easy work.'

(e) The questioning as to the *efficiency of the pause* led in the first research in the majority of cases to mention being made of a relieved, fresher feeling immediately after the pause, which wore off as work proceeded. In the second research, however, it provoked some extremely interesting notes. Thus *D* [after the (M_4) work only] mentioned that after the pauses he felt "refreshed but interrupted," *F* found the pause "produced uncertainty," "stopped the swing," "led to the making of mistakes," etc. *L* remarked that at re-commencing, although the feeling of general strain was relieved, it was difficult to start again. Since these answers were in no way suggested to the subjects there can be no doubt, that although quite young, they must have definitely experienced the remarkable effects of incitation which have appeared so evidently in these investigations. At the same time, it must be remarked that statements of the kind quoted were distinctly exceptional. The majority of the answers refer merely, as in the first research, to a refreshed feeling after the pause.

(*f*) In about a third of the cases examined in the first research, mention is made of slight finger, hand, or muscle-strain, but strange to say quite half of these are in the multiplication tests of four subjects (*S*, *P*, *Y* and *M*), all of whom, it may be remarked, are highly fatiguable or exhibit a tendency in that direction throughout. On the other hand, in the later investigation such physical strain was found to occur in two-thirds of the cases at the (M_2) work, but even then only slightly, while in only one-sixth of the (M_4) observations is any mention whatever made. Apparently therefore we have no cases here similar to those mentioned above.

The results of this introspective examination of the subjects are of course not in any sense complete, nor is very much claimed for them, since the success of the scheme was in the first instance quite problematic, the age of the subjects making it uncertain whether their introspective testimony would be reliable. But nevertheless since so much has been arrived at, there seems distinct ground for hoping that other researches along similar lines may receive augmented value from the adoption of some plan of this type.

APPENDIX B.

A simple method of estimating fatiguability.

If investigations such as the foregoing are to be of real value for pedagogical purposes, they must undoubtedly be carried on on a very much larger scale than those described in the preceding paper. This however, as may be easily conceived, is a matter of very considerable difficulty, for not merely are purely experimental details greatly complicated by an increased number of subjects, but the amount of labour entailed by the calculation of the various results becomes a most stupendous task, the prospect of which is sufficient to appal the most enthusiastic investigator. To such an extent has this fact been impressed upon the author, by the expenditure of time and labour necessitated by researches upon only twenty-four subjects, that attention has been particularly directed to the question of simplifying methods of calculation. Up to the present very little progress has been made, except as regards the method of estimating fatiguability, but here at least there does seem some prospect of diminishing to a very considerable extent the purely arithmetical side of the work. Briefly, the author's suggestion is as follows:

If the more favourable pause is followed by the best work that the subject is capable of, then any deficiency which appears in the pauseless experiment may be regarded as due to fatigue. Why should not this deficiency be regarded as a measure of fatiguability? Thus in the (M_2) pauseless experiment of the second series B lost 8·0 °/₀ in the second ten minutes, over what he had done in the first ten minutes. But in the more favourable pause experiment, he gained 4 °/₀. That is to say, there was a deficiency of 12 °/₀ due to fatigue, and it seems perfectly legitimate to regard − 12 as a measure of his fatiguability. By taking the algebraic difference then between the increase per cent. in the pauseless experiment, and the increase per cent. after the more favourable pause, we obtain very simply a coefficient of fatiguability, which has been found to be quite as accurate as that yielded by the best method yet devised, while involving no questionable mathematical procedure. It will be found to yield both positive and negative values, but when subjects actually do better work without a rest-pause than with one, this is unavoidable, although, as we have already seen, such a condition is quite abnormal.

When it is remembered that on the present system of calculation, we have to perform quite a complicated set of operations for each subject, it is obvious that such a method of estimation effects a vast saving both of time and labour. As an instance of the difference between the two, we may take the case of subject *B* (given above). In the second series he obtained at M_2 work after his more favourable pause an increase of 4·1 °/₀. During the first half of his pauseless experiment he did 459 multiplications, so that but for the unfavourable conditions due to continuous work in the pauseless experiment, we *expect* he would have done 104·1 °/₀ of 459 multiplications, *i.e.* he would have done 478. As a matter of fact he really did 422, which represents a loss of 56 or − 11·7 °/₀. This percentage difference is taken as a measure of his fatiguability. In the preceding paper the weakness of working upon *expected* quantities of work in so reckless a fashion has been pointed out, so that we need not dwell upon it here. The suggested method has of course the additional advantage of avoiding this particularly questionable operation.

As has already been stated, an absolute agreement was found between the results obtained on the two methods, the table given below being a typical example, but although such agreement supports the results given by the longer method, it is certainly no justification for what is after all a slipshod application of mathematical operations.

TABLE XXI.

Shewing the results obtained for fatiguability for twelve subjects, calculated on the two methods.

Subject	Present Method						Suggested Method			Position in both cases
	M F P °/o Gain	1st half 0 pause expect.	Ex-pected	2nd half actual	Diff.	°/o	°/o Gain 0 pause	°/o Gain *M F P*	Algebr. Diff.	
A	9·1	259	282	298	16	5·6	15·1	9·1	6·0	2
B	5·5	331	349	344	− 5	− 1·4	3·9	5·5	− 1·6	8
C	7·7	581	625	603	− 22	− 3·5	3·7	7·7	− 4·0	11
D	9·1	295	322	339	17	5·3	14·9	9·1	5·8	3
E	9·0	378	412	411	− 1	− 0·2	8·7	9·0	− 0·3	7
F	3·9	302	314	317	3	0·9	4·9	3·9	1·0	5
G	11·4	654	728	705	− 23	− 3·1	7·8	11·4	− 3·6	10
H	4·7	432	452	440	− 12	− 2·7	1·8	4·7	− 2·9	9
I	14·2	387	442	487	45	10·2	25·5	14·2	11·3	1
J	15·8	377	436	456	20	4·6	20·9	15·8	5·1	4
K	6·6	360	384	384	0	0·0	6·6	6·6	0·0	6
L	14·8	371	426	399	− 27	− 6·3	7·0	14·8	− 7·8	12

OBSERVATIONS ON SPATIAL CONTRAST AND CONFLUENCE IN VISUAL PERCEPTION.

By W. G. SMITH and S. C. M. SOWTON.

The attempt is made to determine, by the method of reproduction, the character of the modification undergone by a given line—the standard line—through the presence of other adjoining lines. The experimental work falls into four sections:—I. The standard and modifying lines are parallel. II. The lines are continuous, the standard being distinguished from the modifying line by a short crossing line. III. The lines are continuous, but are distinguished by a long crossing line. IV. The standard and the modifying line are successively presented.

Interpretation of the results, with special reference to the conceptions of contrast and confluence. The progressively increasing underestimation of the unmodified as well as of the modified lines. Analysis of the two types of estimation appearing in the course of the observations:—the presence of imagery; the modes of reproductive adjustment. The varying impressions given by the unmodified and modified lines.

While the subject of colour contrast has received ample attention from psychologists and physiologists in the past century, the contrast relations in visual perception of space have been comparatively neglected. It is true that the existence of contrast is acknowledged, and use is made of it as an explanatory factor, but precise investigations regarding its character and its conditions have been very few in number. It was with the view of throwing light on this problem that the observations were carried out which are reported in this paper[1]. The work was limited to the authors of this paper; both took part in preparing material and arranging the experiments; each in turn was subject and experimenter.

---

[1] These observations were the subject of a communication to the Psychological Society on July 14, 1906.

The method of reproduction was used throughout the experiments in the following form. The subject carefully observed the line whose modification was to be studied—which we will term the pattern or standard line—and then, immediately thereafter, indicated, by marking on a sheet of paper, the length of the line which seemed equal to the line previously observed. The length of time to be taken in observing and then in reproducing was left entirely to the subject's discretion, in order that the normal duration of the successive processes might not be disturbed. With the same end in view all movements of the sheet containing the pattern lines, and of the sheet on which the reproduction was indicated, were made by the experimenter who stood at the right hand of the subject. In order that the results might be rendered simple and easily comparable, the pattern line was in all cases of the same length, viz. 10 cm. In every case it is the pattern line which is understood to be affected by the neighbouring lines in the way of contrast, or otherwise. No doubt the modifying lines are themselves modified, but the character of the modification, so far as they are greater or less than the standard line, may be assumed to be in general the same as that of the standard line, which in turn stands to them in the same relations of magnitude.

The work falls into four sections in each of which a special set of conditions is studied. The first section deals with the case where the line which might be supposed to exert the contrast effect—the modifying line—is parallel to the standard line. In the next two sections the modifying line is continuous with the standard line: in the second the distinction between the two lines is marked by a line 2 mm. long, crossing the continuous line at right angles, while in the third the distinction is marked by a crossing-line of 10 mm. In the fourth section an attempt is made to determine whether by this method data regarding successive contrast can be obtained: here the observation of the pattern line is preceded by the presentation of another line of varying length, which is seen for a few seconds in the same position as the line whose length is to be estimated.

In order to get some idea as to the character of the modification by adjoining lines, whether in the way of increase or decrease, the modifying line varied in length from 20 to 2 cm., the intervening lines differing from each other by 2 cm. It was necessary at the same time to have for the purpose of control a determination of the estimated length of the standard line when it was unmodified, *i.e.*, not accompanied by any other line. We can treat this last case as the zero or limiting value of the modifying line, and we can then speak of our results in terms of a series of modifying conditions, eleven in number, varying from 20 cm. to zero. It seemed that the limit of 20 cm. might well be taken as including at some point, or points, a distinct modifying effect, and it will be found that the results which were obtained justify this assumption. It hardly needs mention that each one of the eleven modifications was presented on a different sheet of paper.

The sheet on which the pattern line together with its accompanying line was drawn, was placed on a desk or stand which we kept in the same position throughout, at a constant distance from the edge of the table, the stand being so arranged that the sheet in every case was inclined at an angle of 60° to the table. The sheets on which the estimated length was marked were kept in each set of experiments at a constant distance from the stand, directly between it and the subject, who sat at the edge of the table. These sheets, which were specially prepared for us, were ruled on one side with faint blue lines distant 25 mm. from each other. The width of each sheet (*i.e.* the length of the lines) was slightly under 28 cm.; its length was 28·5 cm., so that there were 10 lines included in the sheet. Before the hour when the observations were made a short line, 2–3 mm. in length, was drawn in ink across each blue line at a point 40 mm. distant from the border of the sheet. This mark formed one end of the line whose length was to be determined; thus the only thing that the subject had to do, after his observation of the pattern sheet, was to make with his pen another mark on this line, whose distance from the first should be equal to the length of the pattern line. As soon as each mark was made the line just marked was removed from the sight of the subject by the following arrangement. Two of the sheets, turned over so as to expose a uniform white surface, were fastened on the table with drawing pins in such a way that the edge of one slightly overlapped the corresponding edge of the other. The sheet containing the lines already marked at one end was then placed with its border under the overlapping edge. The position of the line on which the mark was to be made was unequivocally indicated since it was next to the edge of the white sheet; as soon as the estimation had been completed the sheet was slid an inch further under the overlapping edge; in this way the marked line was concealed and a fresh line brought into position. It was neither desirable nor possible to have the position of the marked line precisely symmetrical in every instance with the position of the group of lines on the pattern sheet, but in each successive set of experiments the relative position was constant and uniform for both observers.

Each of the pattern lines was estimated twice in succession; the sheet containing the pattern line was then removed from the stand and another put in its place. After each estimation the subject looked away, so as to avoid noticing the subsequent manipulation of the sheets. The order in which the different sheets should be presented was determined afresh by lot before each sitting began. When one of the collaborators acted as reagent at the beginning of any sitting, the order was reversed next time, and the position of experimenter was taken. All the eleven cases were presented each day and for the same number of times to each subject; consequently the number of separate estimations at each sitting for each person was either 22 or 44. When 44 estimations were made at one sitting they were not done continuously: after one collaborator had completed 22 estimations the other undertook the work of estimation, and so on. By adjusting the work in these various ways we hoped to avoid having our results biassed by any special arrangement of the experiments. The observations were carried on, usually in the forenoon, at various intervals from November, 1905, to July, 1906, in the Thompson Yates Laboratories of the University of Liverpool. It may be mentioned that when the research was originally planned it was intended to include a wider circle of questions, of methods and of observations. Owing however to the fact that one of us had to change his place of residence, the collaboration on the basis of which the work was commenced had to be terminated. Consequently these experiments must be regarded as the first instalment of an investigation which will now be prosecuted under other conditions.

I. MODIFICATION BY PARALLEL LINES.

For the purposes of this section the lines whose relations were to be examined were drawn parallel to each other, in Indian ink, at a distance of 1 cm., on sheets of stout cream-tinted paper measuring 28 cm. by 25 cm.: the thickness of the lines was approximately 0·35 mm. The standard line held the same position on each sheet: it was drawn parallel to the longer border at a distance of 12 cm. from it: one end was distant 40 mm. from the shorter border of the sheet, this distance, it will be noted, being the same as that of the reproduced lines from the border of the other sheets. The modifying line was 12 cm. from the long border to which it was nearest; one end held a fixed position which was similar to that of the pattern line, and distant 40 mm. from the same border. These points are illustrated in the following figure, in which the short vertical line[1] represents the shorter border of the sheet, and the modifying line is 4 cm. long.

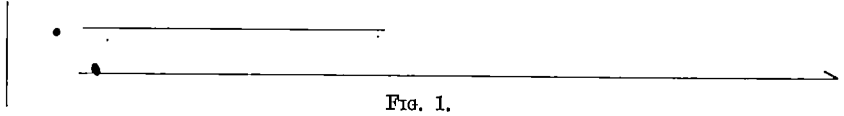


Fig. 1.

For convenience of description the end of the pattern and modifying line, which is at a constant distance from the short border, will be termed the fixed end. It is obvious that many other modes of grouping the lines are possible; they would however have greatly complicated the procedure if they had been utilised; while the arrangement by which the pattern and modifying lines started level, so to speak, from one end seemed to embody with sufficient clearness the relations which were to be investigated.

Our first procedure was to determine the estimated magnitude of the standard line when it was in the horizontal position and was underneath the modifying line: in this set of experiments the fixed end was to the left hand of the subject. Our next step was to repeat the observations with the sheets containing the standard lines reversed so that the standard line was uppermost and the fixed end was on the right. For both arrangements the procedure in reproduction was the same; the line on which the mark was to be made was parallel to the standard line and had its fixed end (*i.e.* the end indicated by a short line 40 mm. from the edge) on the left hand of the subject, so that the right hand could be freely used for indicating the other end. In the third and fourth sets of observations we aimed at determining what character was assumed by the modification when the lines were vertical: in the third set the fixed ends of the standard and modifying lines were underneath, the standard line being on the right hand, while in the fourth the fixed ends were uppermost and the standard line was on the left hand. In these last two sets the line on which the apparent length had to be indicated was at right angles to its former position, and approximately in the same straight line with the standard line; its fixed end was that which was nearest to the subject.

---

[1] The short vertical line representing this border is for practical reasons placed 10 mm. instead of 40 mm. distant from the ends of the horizontal lines.

By these various modes of presentation we hoped not merely to get information regarding the effect of each position, but to be able to eliminate in the final result the influence of any one position. Ten estimations were made in each of the four sets, with each of the eleven different cases of modification. The final average in each of the eleven cases is thus for each individual based on forty estimations. Here and in all the following observations a set of ten estimations was never completed in less than three separate sittings, which, except in a very few instances, were spread over as many days, these days being as a rule separated from each other by one or more intervening days. During each set of observations the subject was ignorant as to the way his estimations were distributing themselves; during the first set in each section we were of course in the dark regarding the trend of the results, but we felt free at the conclusion of each set to consult as to the necessity or advisability of any modification of arrangements in view of the results already secured. We are not inclined to attach any importance to the theoretical possibility that the results might be biassed by such partial knowledge of the course of the experiments as we possessed. Before the commencement of the observations which enter into our records a series of estimations was made in order to afford practice and give familiarity with the details of procedure.

In the first of the tables which follow the data for the four positions have been taken together in order to obtain a general view of the changes in estimation of the pattern line as they are related to the changes in the length of the modifying lines. The final average, it may be mentioned, is secured by first determining the mean of each of the four sets of observations, and then combining these means for the averages which are given in the horizontal columns. In the same way

TABLE I. *Lines parallel.*

Length of modifying line in centimetres	0	2	4	6	8	10	12	14	16	18	20
S. C. M. S. av.	101·7	101·5	102·5	101·7	101·6	102·0	104·6	106·0	104·9	104·0	103·7
m.v.	2·7	2·4	2·8	2·7	2·1	2·9	2·5	3·1	2·2	2·9	2·2
W. G. S. av.	101·6	103·9	103·7	101·8	102·9	102·2	106·4	105·9	105·9	104·7	103·7
m.v.	2·0	2·6	2·6	2·0	2·8	1·7	2·2	2·1	2·5	2·7	2·8

the mean variation was first determined for each set of ten observations, and the average of the four values thus obtained was then entered under the corresponding average estimation. The highest and lowest mean variation for all the 44 sets of observations are 3·9 and 1·3 for S.C.M.S., while for W.G.S. they are 4·8 and 1·3. The corresponding range of probable error is 1·1 to 0·4 and 1·4 to 0·4.

In Table II the results for the horizontal and vertical positions are taken separately, and at the same time the eleven cases of modification are arranged in groups. In the first vertical column (*a*), we have the mean for the two horizontal and two vertical positions with the unmodified line: in the second (*b*), we have the average of the estimations when the modifying lines are less than 10 cm. in length: these various cases, taken together, may be termed the 'short' group. In the third column (*c*) come the estimations where the modifying line is equal to the standard line; in the fourth column (*d*) are the results with modifying lines of 12—18 cm.,—the 'long' group; while in the final column (*e*) are the estimations where the modifying line is 20 cm. in length.

TABLE II. *Lines parallel.*

Length of modifying line in centimetres		(*a*) 0	(*b*) 2—8	(*c*) 10	(*d*) 12—18	(*e*) 20
S. C. M. S.	Horizontal...	98·6	97·7	98·2	100·2	99·7
	Vertical	104·8	106·0	105·9	112·1	107·7
W. G. S.	Horizontal...	102·4	104·9	103·5	106·9	105·2
	Vertical	100·8	101·3	100·8	104·5	102·2

Lastly, in order to have the results before us in graphic form we have arranged the results of Table I in the form of curves in Fig. 2. At the left hand are given the absolute values in millimetres of the estimated lines: underneath are the lengths of the modifying lines in centimetres. The continuous line represents the values of the subject S.C.M.S.; while the interrupted line gives the curve for the subject W.G.S.

It is obvious from the tables and the curves that there is a remarkable similarity in the results of the two subjects, not merely in the absolute values, but in the form of the curves. And when we consider the magnitude of the mean variation and the ratio between it and the probable error, we have good ground for regarding the larger deviations shown in the curves as significant. It is worthy of note that there is not in the case of either subject a sensibly greater regularity in the successive estimations when the presented line is unmodified than when it is subject to varying modification.

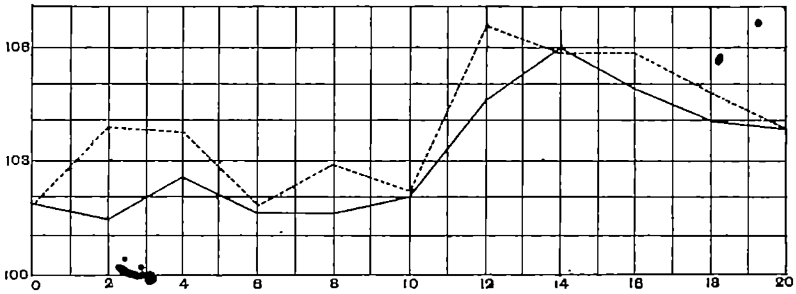


FIG. 2. Parallel Lines.

The general result is plain. While a line shorter than 10 cm. has no constant effect in altering the length of the standard line, a distinct effect is produced when the modifying line is over 10 cm. in length, this effect being in the direction of lengthening. The values presented in Table II show that this lengthening is present whether the lines are vertical or horizontal: inspection of the values for each of the four positions shows that it is present in varying degree in each. This lengthening is, it will be observed, in direct opposition to the ordinary statement of the operation of contrast. The discussion of the various phases of the results will be postponed till we have the data of the later sections before us. Meanwhile reference must be made to an interpretation of these data regarding parallel lines which suggests itself, and which has already been laid by Heymans[1] at the basis of certain of his experiments, with a more complex arrangement of lines. We can it is clear look upon the group of lines as embodying the ordinary Müller-Lyer illusion in which the length of a line is altered by terminal lines directed outwards or inwards: all we have to do is to imagine lines drawn to join the ends of the standard and modifying lines. Were such lines drawn we would have angles of slightly over 26° when the modifying line is 8 cm. long, and of slightly under 154° when the modifying line is 12 cm. long, the joining lines being in each case 22·4 mm. in length. This interpretation, while it satisfies the data of the long group where we have modifying lines longer than 10 cm., does not when taken alone satisfy the data of the short group. It is true that when the angles in this illusion are found on one side only of the main line the illusion is lessened by nearly a third[1]: it has been shown further that the illusion is greater when the accessory lines are directed outwards than when they are directed inwards. Yet these considerations are inadequate to explain the fact that in the short group three out of the four values in col. (*b*) Table II show that the line has actually been lengthened, if we may lay any stress on such small deviations: this difficulty is strengthened by the fact, of which we have assured ourselves, that both subjects are subject in quite sensible degree to the corresponding form of the illusion when angles and lines have the magnitudes given above. This point will be touched on again in the general discussion which will be given afterwards.

---

[1] *Zeitschr. f. Psychol.* 1895, IX. S. 239.

II. MODIFICATION IN CONTINUOUS LINES.

While the ambiguity just referred to must affect all groups of parallel lines we seem to be freed from this particular difficulty when the interacting lines are in the same straight line. In considering the most appropriate arrangement for investigating this point we went on the assumption that, to begin with, we might regard the presentation of the two lines in immediate connexion as the fundamental point, and that the introduction of an interval between the standard and the modifying line would only introduce another variable and complicating factor. In order to mark the point where the standard line ceased and the modifying line began we drew a line 2 mm. long at right angles to the standard line; such a line appeared to be quite distinct and we did not wish, by drawing a longer crossing line, to introduce a factor which, being obtrusive, might distract attention, or separate too distinctly between the standard and the modifying lines. In the following figure the vertical line representing the short border is omitted, while the modifying line in this instance is 2 cm. long. Owing to the fact that the sheets on which the pattern lines were drawn had

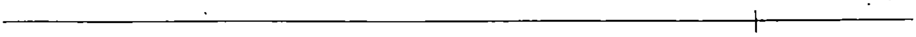


FIG. 3.

---

[1] Heymans, *loc. cit.*; with an angle of 10° and accessory lines of 20 mm., the average combined illusion was 15·5 mm. (S. 229), while with angle and line on one side only the illusion was 11·8 mm. (S. 234).

now to accommodate longer lines than before, their dimensions were altered: in this and the following sections the length of the sheet is slightly over 38 cm., while its breadth is 28 cm. In all other respects the procedure in the successive sets of horizontal and vertical estimations is the same as in the previous section: the results are presented in similar form in Tables III and IV. The range of the mean variations in the various sets of observations is for S.C.M.S. 4·5 to 1·3 and for W.G.S. 3·0 to 1·4, while the corresponding range of probable error is for S.C.M.S. 1·3 to 0·4 and for W.G.S. 0·8 to 0·4.

TABLE III. *Lines continuous: short crossing line.*

Length of modifying line in centimetres	0	2	4	6	8	10	12	14	16	18	20
S. C. M. S. av.	96·0	98·5	99·7	98·7	99·8	98·5	97·7	97·8	96·4	96·8	96·2
m.v.	2·2	2·1	2·4	2·1	2·2	2·2	1·9	2·1	2·4	8·0	2·4
W. G. S. av.	94·1	99·1	98·3	98·2	97·9	97·8	97·1	96·7	94·5	94·4	93·5
m.v.	2·0	2·9	2·8	2·6	1·7	2·8	2·2	2·3	1·8	1·9	1·9

TABLE IV. *Lines continuous: short crossing line.*

Length of modifying line in centimetres		(*a*) 0	(*b*) 2—8	(*c*) 10	(*d*) 12—18	(*e*) 20
S. C. M. S.	Horizontal...	94·5	96·5	96·8	94·1	94·8
	Vertical	97·6	101·6	100·7	100·0	98·1
W. G. S.	Horizontal...	96·7	101·6	99·7	98·4	95·7
	Vertical	91·5	95·1	94·9	92·9	91·2

Here, as in the previous section, there is a close general agreement in the results of the two subjects. The rule applying to both is clear; the presence of a line at the end of the standard line tends to increase the apparent length of the standard line, the tendency being strongest when the added line is 2—4 cm. in length, and lessening until it disappears at 20 cm. These points appear with great clearness in Fig. 4.

The most striking fact, no doubt, is the lengthening of the standard line when the added lines belong to the short group, *i.e.* are less than 10 cm. in length. But the evidence which this fact seems to afford in support of the operation of contrast alters its value when we observe that the same tendency appears, only in a lesser degree, when there is no room for contrast, viz. with a modifying line of 10 cm., and also where the contrast, if effective, should produce shortening, viz. in the long group. That this general result does not appear in the uppermost horizontal line in Table IV is due entirely to the results for the first horizontal position: in all the other positions the rule holds. It is important to note that this lengthening tendency reaches its maximum for each subject when the total line (standard plus modifying line) has the same magnitude as the modifying line which has the greatest effect in Section I. It should be noted also that the curves in Fig. 2 indicate a continuance of the modifying effect beyond 20 cm.—a continuance similar to that found in Fig. 4 from 10 cm. onwards. These relations, which can hardly be regarded as accidental, lend strength to the doubts which were before expressed regarding the interpretation of the data in the first section solely in terms of the Müller-Lyer illusion.

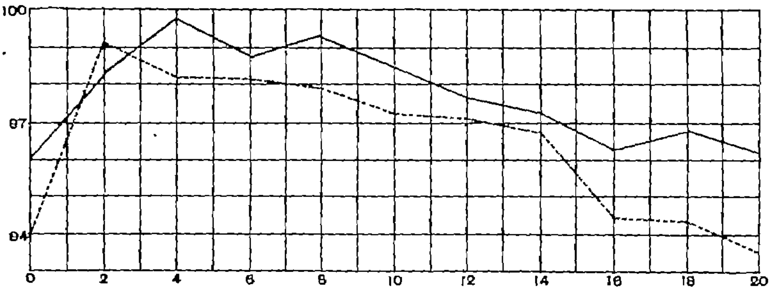


FIG. 4. Continuous lines.

Consideration of the whole results suggests the doubt whether the assumption was justified on the basis of which the experiments of this section were planned, viz. the assumption that the short crossing line adequately marked off the modifying from the standard line. The only solution of this difficulty lay in carrying through additional experiments in which the separation of the two lines should be more distinctly marked, and it is to these experiments that the next section is devoted.

III. MODIFICATION IN CONTINUOUS LINES.

For the purposes of this section a fresh set of sheets were prepared in which the two lines, standard and modifying, were distinguished by a crossing line 10 mm. long. It seemed that, if the results of last section were due to the inconspicuous appearance of the crossing line, this objection could not be made to the new arrangement of lines. The procedure in the experiments was identical with that in former sections, with this exception, that we carried through two instead of four sets of estimations, viz. one in the first horizontal position and one in the first vertical position. We may give here the range of mean variation and probable error: for S.C.M.S. the two are respectively 3·1 to 1·1 and 0·9 to 0·3, while for W.G.S. they are 2·3 to 1·1 and 0·7 to 0·3.

TABLE V. *Lines continuous: long crossing line.*

Length of modifying line in centimetres	0	2	4	6	8	10	12	14	16	18	20
S. C. M. S. av.	92·2	94·0	94·8	96·5	95·3	95·1	96·1	93·6	93·5	93·5	93·9
m.v.	1·5	1·9	1·9	1·7	2·2	1·7	2·3	2·2	1·6	3·0	2·5
W. G. S. av.	91·9	94·5	96·8	96·4	95·4	95·6	94·7	94·5	94·2	93·0	92·2
m.v.	1·7	2·0	1·8	2·5	1·2	2·3	1·7	1·7	1·9	2·3	1·8

TABLE VI. *Lines continuous: long crossing line.*

Length of modifying line in centimetres		(*a*) 0	(*b*) 2—8	(*c*) 10	(*d*) 12—18	(*e*) 20
S. C. M. S.	Horizontal...	90·4	98·1	98·8	92·8	92·0
	Vertical	98·9	97·2	96·8	95·6	95·7
W. G. S.	Horizontal...	93·7	97·2	97·4	95·9	94·6
	Vertical	90·0	94·1	98·7	92·8	89·8

We have as the basis of the values given in the two preceding tables only half the number of experiments which were secured in the previous sections. The results are therefore somewhat less regular, and if they stood alone would not have the significance which they possess following on the experiments of the previous section. But when it is seen that they simply confirm and repeat the rule presented by the previous section in regard to the lengthening effect of the modifying line they are of considerable importance. Here, without an exception, the various horizontal lines of figures in Table VI confirm the general result. In this section however the tendency towards lengthening is strongest with modifying lines which are 2 cm. longer than the corresponding lines in the previous section[1]. We shall probably not go far wrong in connecting this fact with increased practice in grasping the longer lines, and with the consequent increased effectiveness in the modifying action of these lines. In support of this suggestion it may be pointed out that in nearly all the values indicating modification there is an increase in its effectiveness, that increase showing itself not merely in increase of the maximum lengthening, but in the extension of its operation to the region of 20 cm. These points are presented in graphic form in the following figure:

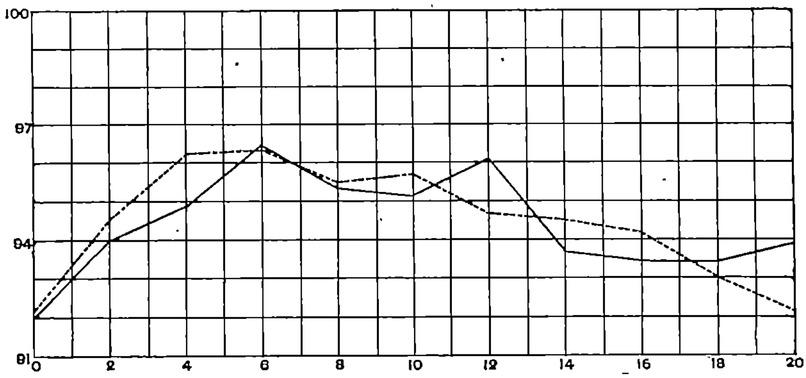


FIG. 5. Continuous lines.

IV. SUCCESSIVE CONTRAST.

In the experiments of this section the sheet containing the standard line was left unchanged in its position on the stand. But at each new estimation, before the standard line was seen, the experimenter placed exactly in front of it on the stand another sheet which had on it either no line at all, or one of the lines having the length 2—20 cm.: the sheet containing this line was removed as soon as the subject gave the signal that he had gained a clear perception of its magnitude. Here, as in the previous section, there are only two sets of observations, one in the horizontal and one in the vertical position. The results do not appear to be sufficiently numerous or definite to allow of our formulating a positive conclusion as to the presence of modification. It is true that with one of the subjects, W.G.S., there is a slight but constant increase in the length of the standard line amounting to 1·2 mm. on the average, when it has been preceded by a line shorter than 10 cm., which is replaced by a decrease amounting to 0·9 mm. on the average, when the preceding line is longer than 10 cm.; the increase and decrease being relative to the normal estimation in which the observation of the standard line was not preceded by the presentation of any other line. The other subject, perhaps, shows traces of a similar tendency, but the results are irregular, both with regard to the absolute values, and the mean variations. In general, however, we may take the data as indicating that it will be possible with more elaborate methods to demonstrate directly the operation of successive contrast. Had the effect of successive contrast in this section, where the conditions for its appearance are relatively favourable, been more marked, this factor might have been utilised to explain the shortening of the standard line in Sections II and III, where it is necessarily preceded in the successive experiments by lines longer than itself. The values for the standard line, when it was not subject to modifying influences, may be given as illustrating the continuation and intensification of the tendency to underestimation which has prevailed in an increasing degree from the first experiments onwards. For S.C.M.S. the values in the horizontal and vertical positions are respectively 84·6 mm. and 94·3 mm., while for W.G.S. the values are 93·7 mm. and 90·0 mm. It may be added that the average length of the reproduced line, in the ten cases where modification was possible, was for S.C.M.S. in the horizontal and vertical positions 87·6 mm. and 94·6 mm. respectively, while the corresponding values for W.G.S. are 92·2 mm. and 90·2 mm.

---

[1] The figures 96·8 and 96·4, in the third and fourth columns Table V, W.G.S. are so close together that we may assume the maximum to be reached in the third column.

We are now in a position to review the results and consider their general bearing. It will be remembered that Helmholtz[1] makes contrast dependent on the overestimation of clearly perceived differences, and considers it a general law for all our perceptions. And it is clear that from the point of view of ordinary experience contrast is understood to be effective in some degree wherever we have a difference in magnitude in adjoining objects. Such an arrangement of lines as is presented in the various cases of Section I would unquestionably be looked on as exemplifying contrast, and it was in part our aim in using such an arrangement to have what would realise in the simplest form the conditions under which contrast is supposed to appear. It is true that linear extensions do not completely represent the ordinary objects of perception, but it can hardly be supposed that this factor, if operative at all, should not influence linear magnitudes. Our results show that both for parallel and for continuous lines the ordinary rule of contrast requires limitation.

---

[1] *Physiol. Optik*, II^{te} Aufl. S. 548.

Evidence in support of this contention so far as regards parallel lines is, it seems to us, to be found in the illusion brought forward by Müller-Lyer[1], which is embodied in the following figure.

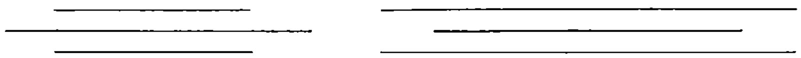


Fig. 6.

It is true that in our arrangement in Section I there are only two lines, and that both modified and modifying lines have one end—the fixed end—in a precisely similar position. But in spite of this it seems that we may regard the rule, which we have found for the arrangement in which the modifying line is longer than the standard line, as being supported by the recognised effect of the illusion shown above.

But if the illusion presented in Fig. 6, be regarded as indicating the same influence, the question, which was touched on before at the end of Section I, still remains; why is there no corresponding effect in our results in the way of shortening the standard line when the modifying line is less than 10 cm. long? In answer it may be pointed out that in such a figure as that given above we have no analysis of the illusion, and consequently do not know how much of the total effect is to be attributed to the disposition of lines on the left hand. There is the still more important consideration that, since the arrangement of lines in our work is widely different from that of the figures given above, the modification might well assume in part a different character. It will in fact be seen later on (*vide* p. 217) that when a line is accompanied by other lines, longer or shorter, it may receive from them an impression of extent such as it does not possess when isolated from all surroundings.

---

[1] Du Bois Reymond's *Archiv f. Physiol.* 1899, Suppl. Bd. S. 268.

There is another factor which deserves mention among the conditions for the special results which we have been considering. When we have a group of parallel lines of such magnitude that the whole figure comes within the compass of clear perception[1], as our lines do when the longest is 10 cm. long, attention will inevitably be attracted involuntarily to the longest line[2]; and this attraction, supported as it is by the voluntary direction of attention, will tend to emphasise the standard line and render the neighbouring lines relatively ineffective. When, on the other hand, the standard line is accompanied by lines longer than itself, the direction of involuntary will not be coincident with that of voluntary attention which is directed to the standard line, and consequently the accompanying lines might be expected to be, as they are, relatively effective. But still the standard line, retaining its constant position on the sheets and having attention persistently directed to it, remains the dominant line and is lengthened throughout by the adjoining lines.

That such cases as we have been examining need to be explained by some factor other than contrast was clear to Müller-Lyer, though the significance of the fact in limiting the statement of contrast has not been adequately recognised. But in regard to another set of cases he has emphasised the operation of contrast as an explanatory principle. We find, in a subsequent paper[3] by him, a statement of the conditions determining whether contrast or confluence shall prevail:—"Extensionen treten in Konfluxion, wenn sie parallel laufen, und sie kontrastieren wenn sie in entgegengesetzter Richtung liegen oder senkrecht zu einander stehen." One of his instances of contrast is given in the following figure, in which the length of the central line appears to vary with variation of the adjoining lines.

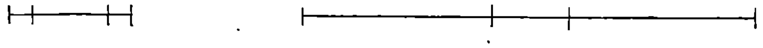


Fig. 7.

---

[1] The image on the retina of a line 10 cm. long would, under the conditions of our experiments, be approximately 3 mm. long: this falls within the horizontal dimensions of the macula.

[2] Cf. Schumann, *Zeitschr. f. Psychol.* 1900, xxiv. S. 81: "Wir sehen also, dass allgemein eine grössere Linie, Distanz u.s.w. vor der kleineren hervortritt."

[3] *Zeitschr. f. Psychol.* 1895, ix. S. 15.

The facts which we have been examining in the second and third sections would naturally fall under the rule regarding 'entgegengesetzte Richtung'; they ought, according to this rule, to exemplify contrast; while in fact they appear to exemplify the rule of confluence[1]. The question then arises: what are the conditions of this apparently opposed result? The size of the crossing line is not the important point, as our experiments in Section III have shown[2]. Nor can it be said that the presence of a larger or a smaller line at one end only of the standard line is the determining factor. Sobeski[3], in investigating the relation of visual to tactual illusions, found that the only form of the illusion represented in Fig. 7 which gave results showing any regularity was one in which the lines that were to be modified were brought into relation with one larger line in one case and with one smaller line in another. And when the cards, in which this arrangement of the illusion was embodied, were presented visually the illusion was found to hold there also, being on the average 5·1 mm. for vision and 7·8 mm. for touch with the same observers. It is to be noted that in these instances the modifying and modified lines were separated by an interval of 1 cm.

On nearer consideration it becomes evident that the opposition between contrast and confluence is superficial and that the modification apparent in Fig. 7 can be accounted for by the processes which have been studied in Sections II and III. The middle line on the left in that figure appears longer, just as our standard line does, because it adds to itself, appropriates so to speak, part of the short lines at each end. The middle line on the right being relatively short does not immediately secure attention; it is the two long lines which are dominant as the middle line was on the left. Consequently it is they that increase in length, while the middle line is shortened. The illusion on this showing falls into line as a case of confluence. Since our work in this paper deals only with lines we cannot do more here than suggest that the cases of contrast in angles, circles and rectangular figures forming a continuous whole may be analysed in the same way: in each case the dominant portion is increased at the expense of the rest of the total figure.

The law of this tendency towards lengthening both with parallel and with continuous lines is made clear by a consideration of the joint result of the first three sections. We have seen already that the modifying line which is most effective in Section I is approximately of the same length as the total line (including standard and modifying lines) in Sections II and III which affords the maximum effect. It will be observed then that the curves expressing this tendency rise, in all the figures, somewhat steeply to this point of maximum effect and that after it they descend gradually, indicating that the effect of this tendency is approaching disappearance in the region of the modifying line of 20 cm.

---

[1] Cf. Lipps, *Raumaesthetik u. geom. opt. Täuschungen*, 1897, S. 86.

[2] It should also be remembered that in Müller-Lyer's figures the crossing lines are quite small, between 1 and 2 mm. in length.

[3] *Über Täuschungen des Tastsinnes*, Inaug. Diss. 1903, S. 47.

We said before, at the beginning of Section II, that we seemed to be freed from the ambiguity of interpreting the results with reference to the ordinary Müller-Lyer illusion when we took up continuous instead of parallel lines. But there is a circumstance bearing on this point which has still to be considered. Heymans finds[1] that this illusion increases from 90° to 10°, reaching its maximum at 10°, when the length of the accessory lines is kept constant; beyond this point no angle was examined. Now the question arises: have we any ground for supposing that the illusion ceases at 10°? Is it not more reasonable to think that the illusion will continue in some degree until we reach on the one hand an angle of 180° and on the other an angle approximating to 0°, until in other words in the one case we have a continuous line like the one we have employed, and in the other approach the plain unmodified line which has served as control in our experiments? Our results give an affirmative answer in part: the line added at the end lengthens the standard line when the two are continuous[2] by an amount which increases with the increase of the added line till the maximum is reached, after which, precisely as in the experiments of Heymans, it gradually decreases. On the other hand it will not be possible to determine how the plain line stands related to lines modified by angles which gradually approach zero until experimental data are furnished. We may point out that in bringing the data regarding continuous lines into relation with those belonging to the ordinary Müller-Lyer illusion our object has not been to interpret the data of Sections II and III as modifications of this illusion, but rather to emphasize the contention that the principle underlying both appears to be the same. Explanation in terms of eye movements can hardly any longer be considered in view of the demonstration by Pearce[1] that the ordinary Müller-Lyer illusion is found in tactual perception. The facts seem to be expressed most simply in the principle urged by Müller-Lyer that in estimation of lines we involuntarily take into account the surrounding space. And we seem to be justified in interpreting this principle as an expression of the general truth that our natural immediate perceptive experience is not primarily analytic and discriminating, but rather continuous and synthetic.

---

[1] *Zeitschr. f. Psychol.* 1895, IX. S. 226. The statement of the results of Table I is not quite in harmony with what is to be found in Tables VII and VIII (S. 231). It appears from these tables that when the appropriate accessory lines (*Schenkel*) are employed the maximum illusion is to be found with an angle of 80°.

[2] Cf. Brentano, *Zeitschr. f. Psychol.* 1892, III. S. 355 n.

We have already had occasion to refer to the progressive intensification of the tendency to underestimation. The decrease in the length of the unmodified line, when we compare corresponding sets of observations in the first and fourth sections, amounts to 14·1 mm. in the horizontal and 8·4 mm. in the vertical position for S.C.M.S.; while it amounts to 13·6 mm. in the horizontal and 8·4 mm. in the vertical for W.G.S. We know of no other factor to adduce in explanation than the tendency, of which we became distinctly conscious in the later stages of the work, to look on the standard line, which, it will be remembered, was kept constant throughout, less attentively, to carry on the estimations more mechanically[2]. The reason why the vertical lines alter less than the horizontal is possibly to be found in the fact that the estimation of vertical lines required a more erect attitude and a greater physical and mental effort: such an estimation would become mechanical or automatic less rapidly. Taking this mechanical tendency as the determining condition of the change in estimation, we should then have to say that the judgment of spatial magnitude is modified profoundly by the familiarity or commonness of the particular objects dealt with. That the phenomenon has nothing to do with any special behaviour of the unmodified standard line is made evident by a glance at the figures: the whole of the curves are above the horizontal line of 100 mm. in Fig. 2: they nearly touch it in Fig. 4; they are distinctly below it throughout in Fig. 5. The same fact appears in the results of Section IV, which are not presented in tabular form: the standard line, as we have seen, has practically the same apparent length throughout the various cases.

---

[1] *Ueber den Einfluss von Nebenreizen auf die Wahrnehmung*, 1903, S. 27, also *Arch. f. d. ges. Psychol.* 1903, I.

[2] Although the mean variation does not form direct evidence on this point, it may be mentioned that the average mean variation in the first three sections is for S.C.M.S. successively 2·6, 2·3, 2·1, and for W.G.S. 2·4, 2·2, 2·1.

It might be suggested that these and other points are due to the fact that the method was not one of simultaneous comparison, but of successive adjustment. Such an influence of the method is possible, but it does not seem in any degree probable that the changes in one direction and another should be due merely to the employment of this special method. And even if the suggestion were regarded as tenable it would only push the problem one step further back: we should then have to explain why the process of reproduction and successive adjustment was subject to such variations.

It will be observed that while the subject S.C.M.S. in every instance exemplifies the overestimation of the vertical as compared with the horizontal line, the opposite is the case with the other subject, who in every case draws a longer line in the horizontal than in the vertical position. Seeing that the latter subject was subjectively aware of the apparent increase in length in the vertical position the explanation becomes somewhat difficult. The explanation of this fact may possibly be traced to an overestimation not merely of the vertical lines on the pattern sheet but of the lines on the sheet containing the reproduced lines; even though this sheet lay on the table, the relation of its lines to the movements of the eye was similar to that of the pattern sheet.

We had an opportunity during the course of these experiments of observing somewhat minutely the processes by which the estimation was carried out. When once the observation of the lines on the pattern sheet was completed we did not look up to compare the line we had drawn with the standard line even when we were dissatisfied with the reproduction. In the course of the experiments we were struck by the entire absence of an image or mental picture of the line during the determination of the length of the reproduced line: in this respect we can only, so far as our work goes, confirm the view, upheld by Schumann[1] and others, that in the comparison which is involved in the judgment that two successive impressions are equal, or different, a *Gedächtniss-* or *Vorstellungsbild*, is not essential and is normally absent. The whole mental activity appeared to us as something whose beginning and end—on the one hand visual impression, and on the other the impulse to make a mark at a given position—were present in consciousness, but whose intermediate and conditioning processes were hidden from us. In this it resembles closely those dexterities, such as writing, of which as a rule the only portions accessible to consciousness are the visual, or auditory impressions, and the series of appropriate movements, which we suppose to be connected with them by intervening cerebral processes.

---

[1] *Zeitschr. f. Psychol.* 1902, xxx. S. 241.

Two distinct types of reproductive process were represented in the work of the two subjects. The one type shows a direct uninterrupted observation of the line and an immediate unhesitating indication of the length of the line which is meant to be equal to the pattern line. Occasionally this indication is followed by the consciousness that the line is too short or too long; as a rule this was absent and the total process may be summed up as a direct perceptive adjustment.

The other type exhibits as a rule an observation in which the eye is carried up and down the lines, which is altogether more deliberate and which is occasionally interrupted by a train of irrelevant ideas that leave one gazing at the line, with the consciousness that in spite of the gazing the line has not been grasped at all. This process of 'grasping,' 'getting hold,' is quite definite: one knows when it is achieved and when it has failed; its explanation probably lies in a consciousness that the process of attention has received full and interrupted development. Occasionally after the observation had gone its normal course there appeared, as the reproduction commenced, the conviction that the line had been completely forgotten. Such a condition has already been described by Müller[1] as occurring in the process of estimating weights. We have noted eight cases[2] where this forgetting was specially prominent and have observed the extent to which each estimation differed from the average of the other observations in the set to which it belonged; the result is that for these cases we have an average deviation of 2·2 mm., or, if we omit the last case where the deviation was excessive, we have a deviation of 1·5 mm. It is clear from these values that in spite of the pronounced forgetting the actual reproduction was on the whole better than the average, *i.e.* nearer to the type of reproduction at that particular stage.

The actual process of indicating the length of the reproduced line was also for this subject of an indirect and mediate character. Relatively seldom was the pen carried to the right point without hesitation:

---

[1] *Zur Analyse der Unterschiedsempfindlichkeit*, von L. J. Martin u. G. E. Müller, 1899, S. 48, "Ich bin erstaunt darüber, wie oft ich bei der zweiten Hebung eines Versuchs gar keine Erinnerung mehr von dem Eindrucke habe, den das Gewicht der ersten Hebung gemacht hat."

[2] We included these cases among the estimations on which the Tables are based, chiefly because if we had rejected them we should have been forced in repeating the observations to spend more than the usual length of time on particular cases and should have thus interfered with the regular course of the experiments.

the usual process was tentative, experimental; the pen was carried backwards and forwards in the region of the proper point until some definite position appeared satisfactory. Sometimes the method was adopted—naturally without premeditation—of moving the pen outwards until the response 'too long' appeared, then retracing the line till the position appeared to be 'too short' and finally taking up a position which, as being intermediate, was assumed to be 'right.' One other detail may be mentioned: as the observation of the pattern was being completed there occasionally formed itself the judgment 'long' or 'short'—if we may employ this term to indicate the actual process, which as a matter of fact was felt rather as an association. This consciousness then directed the reproductive process, so that the line was made to conform to the requirement 'long' or 'short,' but immediately afterwards there arose the reflective and inhibitory consideration, 'surely the real line was not thus "long" or "short".' Upon the more automatic realisation of the idea there followed the corrective critical appeal to memory of the actual impression.

It might be thought that this subject, since the whole process was so hesitating and uncertain, would have a much larger mean variation than the other subject, while at the same time the absolute values of the estimations might be expected to diverge somewhat widely from those of the other subject, whether in the direction of greater nearness to the standard line, or in the reverse direction. As a matter of fact the mean variation of this tentative hesitating method is in all the sections on the average less than it is with the method of direct perceptive adjustment. As regards the absolute values, the answer is seen at once on glancing at the curves: in spite of the diversity in mode of estimation the absolute values and the form of the curves as we have seen already show a close correspondence. With respect to the conclusions which we have drawn this variety of method is a matter of satisfaction; it proves that the relations which have been found to hold are independent of the special mode of estimation.

It was interesting to observe how in the later stages of our work the unmodified standard line frequently presented itself with an almost emotional tone or quality as being lonely, shrinking, small. We have to do here it would seem with an 'absolute impression' of shortness such as has been insisted on by Martin and Müller[1]. When we inquire for the special conditions determining the appearance of this impression apart from the progressive underestimation, a reasonable suggestion seems to be that which brings the fact into connexion with the data already brought forward regarding the effect which neighbouring lines have on the judgment of magnitude of a given line. We have in a few of the cases studied seen a diminution of the standard line through the introduction of adjoining lines: we have on the other hand usually found an increase. It would seem then that the absolute impression of shortness is conditioned in part by the absence of such lines: when the adjoining lines, parallel or continuous, are gone, the sense of extension is replaced by a sense of unsupported isolated magnitude, which tends to determine the judgment of length in the direction of shortening. This factor would coincide in its action with confluence in all cases except those in Section I, where the adjoining lines are shorter than 10 cm., and it is here that the action of confluence disappears. One other feature in the perception of the standard line may be referred to—the undulation or alteration in the sense of its length as one continued to gaze at it with the view of securing a firm grasp. It seems probable that the main cause of this undulation is an alteration in the character of the innervation of the eye muscles and more particularly of the ciliary muscle.

---

[1] *Loc. cit.* S. 43; also *Die Gesichtspunkte u. d. Tatsachen der psychophysischen Methodik*, SS. 113, 124, and Schumann, *Zeitschr. f. Psychol.* xxx. S. 255.

GENERAL CONCLUSIONS.

1. The presence beside a standard line of 10 cm. of a modifying line parallel to it and longer than 10 cm. tends to increase the apparent length of the standard line by an amount which increases up to a certain point with increase of the modifying line and then gradually diminishes.

2. When a standard line of 10 cm. is joined by another modifying line continuous with it, but distinguished from it by crossing lines of different lengths, the standard line is apparently increased by an amount which increases up to a certain point with increase of the modifying line and then gradually diminishes.

3. The modifying line which produces the greatest effect when the lines are parallel is approximately of the same length as the total continuous line which shows the greatest lengthening of the standard line.

4. These two forms of modification are considered to be of essentially the same character and to exemplify the process of confluence.

5. The estimation of length of the unmodified standard line begins with approximate equality between it and the reproduced line; in the subsequent estimations there appears in the work of both subjects an underestimation which gradually increases; it amounts to about 10 per cent. of the standard line in the final observations.

6. In employing the method of reproduction two distinct types of estimation became apparent; one of these shows a direct perceptive adjustment, while with the other the adjustments are of a tentative and hesitating character. It is found, however, that the estimations of the latter form are less variable throughout.

7. In the process of forming a line equal to the one just seen the presence of an image or '*Gedächtnissbild*' could not be detected.

THE VERTICAL-HORIZONTAL ILLUSION IN SCHOOL-CHILDREN.

BY W. H. WINCH.

I. The problem stated. II. Method of experiment.
III. Tables of results. IV. Suggested conclusion.
V. The bearing of the results on the probable causes of the erroneous estimation.

I. SOME ten years ago, in the course of a consideration, under the direction of Dr W. H. R. Rivers[1], of the visual illusions known to psychologists, I found myself unexpectedly acquainted with them, having seen them exemplified in a very marked way in the drawings of school-children.

Since then, work on visual illusion among primitive peoples has given definite and consistent results; but it remains to be seen how far the civilised child passes through the stages of development now exemplified in the savage.

From a pedagogical aspect, too, the nature and extent of spatial illusion is of great importance. In the accuracy of its proportions lies much of the excellence of good drawing; and teachers frequently call the attention of their pupils to the relations between the height and breadth of the figure or object they are drawing. When it is remembered that such a comparison almost always involves error, and that too, in the same direction, it becomes most necessary to investigate its nature and extent. The following work is a contribution to this investigation.

II. The problems set were as follows:—

Given a horizontal line 5 inches long.

First form. To draw a vertical line equal to it upwards from its middle point.

---

[1] *Report of the Cambridge Anthropological Expedition to Torres Straits,* Vol. II. Pt. I. p. 116, by W. H. R. Rivers.

Second form. To draw a vertical line equal to it upwards from its right extremity.

Third form. To draw a vertical line equal to it through its middle point so as to form a cross.

Each child was provided with papers 7¼ inches square, with the horizontal standard line drawn about 1 inch from the bottom for the first and second forms of the exercise, and near the middle for the third form. No child was permitted to twist his paper in any way, but was allowed to correct his judgment by addition to this vertical line, or by erasure, if he thought he had drawn it too short or too long. Before setting down and turning over each drawing as finished, he was required to hold his paper nearly vertically before him in two hands with arms outstretched and make a final judgment. I carefully noted the alterations made in this final estimate, but found no general tendency either to lengthen or shorten at this point.

III. In the case of School "D" the children were selected from seven classes ranging from Standard II to Standard Ex-VII. Care was taken to obtain boys from each class who should fairly represent it in age and attainments, and should show a wide variation in general capacity, whilst, at the same time, the mean position of the group should work out to about 50, on the supposition that the class contained 100 children.

The 4th, 8th and 12th columns in Table I express the lengths of the vertical lines drawn, in percentages of the standard horizontal. The 7th, 11th and 15th columns give the order of estimated vertical lengths, beginning with the shortest.

In the case of school "K" only six exercises were obtained, as the school time had expired before the third form of the exercise could be drawn. Six pupils were tested in each standard and they were selected from their classes so as to be properly representative in age and general proficiency. Their average estimations are given in Table II.

As there might be some doubt whether the boys chosen were representative of their standards, I also took, in another school, two whole classes, Standards II and VI, giving them only one exercise for each form of the figure instead of three (See Table III).

A fourth series of exercises was given to a number of adults who had received the usual education of well-to-do English people (Table IV). The estimations are more accurate than those of the lower standard school-boys, but less so than those of the upper standards.

TABLE I. SCHOOL "D."

Showing the estimations of the vertical as percentages of the standard horizontal.

				First Form				Second Form				Third Form			
Standard	Name Initials only	Age on 31 7 06 Yrs.	Mths.	Average of 3 Exercises	m.v.	Order in Age	Order of Estimation of Verticals	Average of 3 Exercises	m.v.	Order in Age	Order of Estimation of Verticals	Average of 3 Exercises	m.v.	Order in Age	Order of Estimation of Verticals
II	F. J.	8	7	79·0	4·6	1	7	88·0	1·3	1	8	78·3	2·4	1	2
,,	D. A.	8	9	86·0	2·6	2	16	92·6	4·8	2	14	78·3	4·9	2	2
,,	B. J.	8	10	82·6	·8	3	12	89·0	2·0	3	11	100·6	3·5	3	37
,,	F. L.	9	1	88·3	2·2	4	20	103·3	1·7	4	38	97·6	4·4	4	28
,,	B. A.	9	1	72·0	2·0	4	5	83·6	2·8	4	4	94·0	4·0	4	21
,,	S. D.	9	1	70·6	3·5	4	4	81·0	1·3	4	3	80·3	·4	4	5
	Averages	8	10	79·7	2·6	—	—	89·6	2·3	—	—	88·2	3·2	—	—
III	S. W.	9	5	99·0	·6	7	37	97·0	1·3	7	21	86·6	1·8	7	8
,,	R. T.	9	8	66·0	1·3	8	1	87·6	3·1	8	7	79·3	6·2	8	4
,,	C. E.	9	11	66·0	2·6	9	1	75·3	3·1	9	1	84·6	2·2	9	6
,,	T. E.	10	0	112·6	5·8	10	42	105·6	1·5	10	40	96·0	2·6	10	25
,,	B. J.	10	2	81·0	2·0	11	11	88·0	1·3	11	8	87·3	·9	11	9
,,	W. A. ...	10	6	80·0	1·3	12	9	106·0	2·6	12	41	107·0	2·0	12	42
	Averages	9	11	84·1	2·2	—	—	93·2	2·1	—	—	90·1	2·6	—	—
IV	S. N.	10	7	89·0	·6	13	23	93·3	1·7	13	16	93·3	2·4	13	16
,,	B. A.	10	8	79·3	3·5	14	8	101·3	4·4	14	34	93·0	6·0	14	15
,,	H. J.	10	10	95·6	1·1	15	32	96·3	·4	15	20	93·3	·9	15	16
,,	B. L.	11	1	88·6	4·8	16	22	98·6	3·5	16	24	101·3	2·2	16	39
,,	S. D.	11	3	89·3	6·9	17	24	83·6	2·4	17	4	91·6	2·2	17	13
,,	W. G. ...	11	5	80·6	1·5	18	10	88·3	1·7	18	10	86·3	·4	18	7
	Averages	10	11	87·0	3·0	—	—	93·5	2·3	—	—	93·1	2·3	—	—

V	S. E.	11	7	69·6	2·4	19	3	75·6	3·8	19		77·0	1·3	19	1
,,	B. A.	11	10	96·6	2·4	20	34	106·0	2·6	20	41	97·6	2·4	20	28
,,	P. G.	11	10	88·3	2·4	21	20	98·6	1·8	21	24	93·6	1·1	21	18
,,	C. A.	12	0	87·3	1·5	22	17	98·0	2·6	22	28	97·0	·6	22	26
,,	C. H.	12	2	99·6	·4	23	38	100·0	·6	23	30	100·0	·6	23	34
,,	L. A.	12	2	78·6	2·2	23	6	93·0	·6	23	15	88·6	2·2	23	12
	Averages	11	11	86·6	1·8	—	—	95·2	2·0	—	—	92·3	1·3	—	—
VI	S. H.	12	6	103·3	2·2	25	41	102·3	1·1	25	35	98·3	3·4	25	31
,,	S. N.	12	8	85·6	4·2	26	15	94·6	·8	26	18	91·6	1·1	26	13
,,	P. L.	12	10	91·0	2·6	27	28	97·3	1·1	27	22	93·6	4·2	27	18
,,	C. R.	13	4	89·3	2·2	29	24	94·0	·6	29	17	93·6	1·8	29	18
,,	E. R.	13	4	94·6	·8	31	31	100·3	1·1	31	31	95·0	·6	31	22
,,	C. H.	13	5	85·0	3·3	31	14	92·3	1·1	31	13	95·0	·6	31	22
	Averages	13	0	91·4	2·5	—	—	96·8	·9	—	—	94·5	2·0	—	—
VII	R. S.	13	8	98·3	4·2	28	35	102·6	2·2	28	37	100·3	4·2	28	36
,,	D. C.	13	6	87·6	9·4	32	18	99·0	·6	32	26	95·0	1·3	32	22
,,	P. S.	14	1	84·3	1·7	33	13	87·0	2·0	33	6	87·3	5·7	33	9
,,	S. H.	14	4	90·3	·4	35	27	95·6	·4	35	19	98·3	2·9	35	31
,,	R. W.	14	7	90·0	2·6	36	26	99·6	·4	36	29	98·0	1·3	36	30
,,	W. C. ...	14	9	102·6	·8	37	40	99·3	·9	37	27	104·6	3·3	37	41
	Averages	14	1	92·2	3·1	—	—	97·2	1·0	—	—	97·2	3·1	—	—
Ex VII	S. H.	14	8	95·6	1·5	34	32	99·3	·4	34	27	99·0	·6	34	33
,,	E. J.	14	9	94·0	2·6	37	30	100·3	1·5	37	31	87·6	2·2	37	11
,,	T. M.	14	9	99·6	1·5	37	33	102·3	1·1	37	35	100·0	·6	37	34
,,	G. O.	14	11	87·6	2·8	40	18	90·6	2·2	40	12	97·0	2·0	40	26
,,	C. J.	15	1	98·3	2·9	41	35	103·6	1·1	41	39	103·3	1·1	41	40
,,	P. R.	15	11	91·6	1·1	42	29	100·6	·8	42	33	101·0	·6	42	38
	Averages	14	11	94·4	2·0	—	—	99·4	1·1	—	—	97·9	1·1	—	—

TABLE II. SCHOOL "K."

Showing the estimations of the vertical as percentages of the standard horizontal.

Standard	Average age	First Form	m.v.	M.V.[1]	Second Form	m.v.	M.V.
II	8 yrs. 9 mths.	72·5	3·8	6·8	91·4	2·2	5·7
III	10 ,, 1 ,,	77·9	3·8	4·8	91·0	2·4	4·7
IV	11 ,, 2 ,,	83·3	3·1	8·7	90·3	1·9	7·0
V	12 ,, 0 ,,	86·7	3·1	1·8	95·9	2·0	4·2
VI	13 ,, 1 ,,	87·0	2·4	1·8	99·9	1·0	5·2
VII	13 ,, 11 ,,	88·8	2·6	4·5	99·7	2·0	2·6

TABLE III. SCHOOL "S."

Showing the estimations of the vertical as percentages of the standard horizontal.

	Average estimation of 54 Standard II children, average age 9 yrs. 6 mths.			Average estimation of 86 Standard VI children, average age 13 yrs. 4 mths.		
	1st form	2nd form	3rd form	1st form	2nd form	3rd form
Av.......	79·8	91·2	89·0	91·0	97·6	95·1
M.V. ...	6·1	4·8	4·9	4·7	3·6	4·6

TABLE IV. ADULTS[2].

	Average estimation of 9 adults, average age 24 yrs. 4 mths.		
	1st form	2nd form	3rd form
Av.......	84·3	92·8	96·9
M.V. ...	10·1	5·6	4·1

IV. A careful perusal of the foregoing results will suggest the conclusion that, so far as school-children are concerned who are trained to draw, there is a gradual diminution in the amount of the illusion as the children rise in age and standard. The results in Table I are so arranged as to permit of the easy application of correlation formulae, and the reader is referred to Dr Spearman's 'foot-rule'[3] as an expeditious way of obtaining a numerical expression for correlation.

It will be noted that the second form of the illusion is, with these children, less marked than the third, whilst this is not usually the case with adults. The drawing of the second form is one in which every child has had some training, since it is in this way that children begin to draw a square; in the case of the two other forms no direct instruction had been given.

---

[1] The M.V. is the average deviation of the individuals from the mean of the group. The m.v. is the average mean variation showing the steadiness of the work of each group of pupils. It corresponds to the average m.v. given in Table I.

[2] These results should be compared with those given in the *Torres Straits Reports*, by Dr Rivers, p. 113.

[3] This *Journal*, Vol. II. p. 89.

V. Various explanations have been put forward as to the cause of this 'illusion' or erroneous estimation. The term 'illusion' seems hardly satisfactory, since the error is apparently a feature of normal development. Perhaps the broadest division as to suggested causes lies in the grouping of the illusions as either physiological or psychological. What is meant by this distinction may perhaps be also expressed by the difference between central and peripheral. In any case, if training of the kind usually given in drawing suffices to diminish the illusion, we can at least be sure that we are not dealing with some wholly ineradicable physiological factor entirely removed from intellectual control; for we come at last to 'see' the length rightly as well as to 'know' that we used to estimate it wrongly.

The gradual elimination of error seems to be a singularly interesting example of what appears to me to be a backward and forward play of sensation and thought. One begins with two sensations whose spatial quality, except as to direction, seems identical; and one judges the shorter vertical to be equal in length to the longer horizontal. The error is pointed out or discovered and, finally, a right, or much more nearly right, judgment is made. Has the sensation changed or the judgment only, leaving the sensation unaffected? It is possible, doubtless, to say that the two lines are 'sensed' from the first just as they are afterwards, and that it is only the thought of comparison which undergoes a change. But does this appear to be so? At first, the shorter vertical seems to give as much feeling of length as the longer horizontal; but, bit by bit, one's feeling of the length becomes more and more in accordance with one's knowledge of it, till, at last, one comes not only to judge but to 'feel' the length rightly. This, it seems to me, may involve change of sensation, which may be, at least partly, brought about by a change of conception. If this position is valid, sensations not only partly determine, but are partly determined by thought.

I do not propose to enter into the vexed questions of the influence either of varying retinal curvatures in different planes through the eye, or of the greater effort required for muscular movement in the vertical as compared with the horizontal plane, or of the shape of the field of binocular vision. But whatever factors are operative, they must, if the above conclusions be well based, be such as diminish in influence with the normal growth and progress of a school-boy through his school.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

November 3, 1906.	A List of Mental Characters for Anthropometric Investigations in Schools, by W. McDougall. (See *Rep. Brit. Ass.* 1906, pp. 352–3).
December 8, 1906.	Continuation of Discussion from November 3.
January 26, 1907.	On Sensation, by C. S. Myers. A New Visual Illusion of Direction, by James Fraser.
March 9, 1907.	Experiments on Fatigue, by Mrs Reid and Miss Martyn. One of the Functions of Rhythm, Rhyme and Assonance in Poetry, by P. J. Hartog.

VOLUME II JANUARY, 1908 PART 3

THE METHOD OF 'RIGHT AND WRONG CASES' ('CONSTANT STIMULI') WITHOUT GAUSS'S FORMULAE.

By C. SPEARMAN.

1. *Need of dispensing with the Gaussian formulae in the method of 'right and wrong cases.'*
2. *Proposed general procedure.*
3. *First method of measuring the average limen.*
4. *The case of equal intervals.*
5. *Second and slightly more accurate method of measuring the average limen.*
6. *Mean deviation.*
7. *Conclusion.*

1. *Need of dispensing with the Gaussian formulae.*

GAUSS, as is well known, discovered that the fluctuations of 'chance' errors of measurement are regulated by a mathematical law. His formulae were welcomed by scientists, met with remarkably close empirical corroboration and contributed not a little to the exactness of modern physical and psychical research. But of late years opinion would seem to be veering round; the use of the formulae encounters a continually increasing opposition, especially in psychology.

This opposition is, the present writer believes, sound enough in general principle, but defective in execution. It contents itself too easily with reiterating that deviations from the formulae are conceivable and actually occur, instead of determining the particular circumstances under which such deviations must and do take place; it is too eager to cast aside the formulae altogether because they are occasionally misleading, instead of first seeing if there are not also occasions where these formulae render right good service. Now, Gauss's law deals—by the terms of its demonstration—only with such variations as are the additive result of very many independently varying and very small factors[1]. And accordingly, it has shown itself to fit admirably just those variations which may not unreasonably be regarded as the accumulative effect of factors of such a kind (notably, the fluctuations of the spatial measurements of physical science). But the law ought never to have been expected to apply accurately to any variations where the above two conditions, the independent additive effect and the relative smallness of the factors, are known not to be fulfilled.

The second of these conditions is of more interest for psychology; beyond all question, the factors influencing the observed variations are in psychology often relatively *large*[2]. The measurements taken of a person are perturbed by practice, fatigue, 'absolute impression,' attention, suggestion, confidence, etc. Those of a class are differentially affected by natural selection, artificial elimination, mixture of species, external limits and a host more. These relatively large factors inevitably cause more or less disagreement between Gauss's 'normal' distribution of values and that actually observed; and such disagreements are daily becoming less tolerable, as psychology is growing into a more and more exact science.

Now, the question of these disagreements is in a peculiarly acute stage as concerns the admittedly most scientific method of psychophysical measurement, that of 'right and wrong cases' or, as it has recently been termed, of 'constant stimuli.' For it is beginning to be realized that this method is applicable to a very extensive field over and beyond the determination of limina, to which it has hitherto almost exclusively been confined[3]. Further, these liminal determinations, formerly cultivated with such zeal and then suffered to fall into neglect, are now beginning to be taken up again with something of the old interest but with a new purpose.

[1] It is sometimes but incorrectly said, that these factors also need to be all equal in amount and all equally probable.

[2] As regards the first condition, to postulate that the factors should be independent and additive is to postulate the independence of their respective effects upon the total variations—expressed more mathematically, the independence of the partial differential coefficients of the total variable, taken with respect to the part variables. But it should be remembered that, even should this independence really exist when choosing the 'argument' rightly, it will no longer hold good when using any other argument not a linear function of the right one. This fact is not taken into consideration by those who think to refute Gauss's law simply by pointing out, as they believe, its discrepancy from observation. To take one instance among many, whenever absolute measurements are wrongly substituted for relative ones, such a wrong argument is not a linear but a logarithmic function of the right one.

[3] As far as I can see, it is applicable to the whole range of judgments that depend on stimuli admitting of fine gradation.

For it has lately been discovered, that just those anomalies of experimental results, which so grievously obstructed all attempts at accurate verification of the law of Weber, will bring in a rich psychological harvest when investigated on their own account. But this involves work of the utmost delicacy and exactness; the disagreements between the 'normal' and the actual distribution of values, far from being considered too small to be taken into account, must form one of the chief objects of investigation. Hence, we are now in great need of a method whereby to measure this actual distribution.

This does not at all mean that the 'normal' distribution is to be thrown away like a worn-out garment. On the contrary, it still applies to a vast number of cases with sufficient approximation to do great service in research (this service, so precious to the researcher, is sometimes ignored by the mathematician 'at the green table'). And even when the 'normal' distribution ceases to indicate closely the factual one, it at any rate remains an invaluable standard for estimating the latter; the very discrepancy between the two furnishes the principal means of investigation.

2. *Proposed general procedure.*

As it is required to measure the actual distribution—whatever this may turn out to be—of the values investigated, it is of course necessary that the experimental data should suffice to determine this actual distribution. This means that the 'stimuli of comparison' must be at small intervals throughout the whole region within which any of the observer's judgments are likely to be undecided; but it is not necessary, although convenient, that these intervals should form a regular series (arithmetical, geometrical, etc.).

To illustrate our procedure, we will take a series of experimental observations by Merkel[1], both as having been very carefully executed and as having been searchingly discussed by G. E. Müller[2]. In these experiments, the observer had to judge which of two noises appeared to be the louder[3]. First came the 'standard' noise, which we will call S, and whose intensity was calculated by Merkel at the value 1772. Then followed the 'comparative' noise, which we will call C, and which had 11 different intensities varying not quite regularly from 1078 to 3011; with each of these intensities 100 judgments were made. Table I. shows each value of $C-S$, which we will call D, together with the ensuing amounts of the three different kinds of judgments, 'greater,' 'undecided,' and 'less.'

---

[1] Wundt's *Philosoph. Studien*, Bd. IV. S. 141.

[2] *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik*, 1904, S. 52, and later.

[3] The noises were caused by dropping brass balls on to hard blocks of wood. Following Müller, Merkel's ascending and descending series have here been added together, as they show no appreciable discrepancy from one another.

TABLE I. *The data given by Merkel.*

$D\ (=C-S)$	Judgments as to intensity of C 'Greater' $(=g)$	 'Undecided' $(=u)$	 'Less' $(=l)$
− 694		2	98
− 588		11	89
− 370	11	14	73[1]
− 195	28	20	52
0	48	24	28
+ 200	71	18	11
+ 397	83	15	2
+ 603	91	8[1]	
+ 807	95	5	
+ 1021	98	2	
+ 1289	100		
	$625\ (=\Sigma g)$	$119\ (=\Sigma u)$	$353\ (=\Sigma l)$

We will now consider how these data may be made to furnish the measurement, say, of the upper limen; by this will be understood, as usual, the exact value of D necessary, in order that the observer should just balance between judging C to be equal to or greater than S. Our considerations however, as will readily be seen, are equally applicable to any other sort of limen or overlimen, quantitative or qualitative, absolute or differential; further, to the wide range of other psychological problems amenable to the method of constant stimuli, as the measurement of an optical illusion, the determination of colours in peripheral vision, or that of the most pleasing musical intervals, etc.

It must be borne in mind that the experimenter has only to deal with the actual judgments and the actual physical stimuli; also that the judgments elicited by any given stimulus are not constant but *variable*. Any observer is liable at one moment to judge C greater than S when it is objectively nearly equal, and at the next moment to judge it equal when it is objectively far greater. The conception of an underlying constant limen that would occur on elimination of all variable disturbances is (when not a mere auxiliary hypothesis, for use at a pinch) highly speculative; it is a luxury to be reserved—together with the question, as to whether the causes of variation lie in the stimuli, in the sensations, or in the apprehension of their difference—for the theoretical discussion at the close of proceedings. So long as any investigation is still at the experimental stage, 'the' limen must be taken as only an abbreviated expression for the *mean* limen.

---

[1] In these cases, the number of judgments would appear not to have been exactly 100, but this slight difference will here—for convenience of calculation—be neglected.

Now, it is evident (being implied in the meaning of the words), that on every occasion when the judgment is a *g*, the (upper) limen is some value less than the D then used[1]. To take an instance from Table I., when D amounted to + 200, the percentage of *g*'s was 71; from this we conclude that 71°/$_0$ of the limina were less than + 200. Hence, under the given experimental conditions generally, 71°/$_0$ must be taken as the most probable percentage of limina less than + 200[2]. Similarly, 83°/$_0$ must be taken as the percentage of limina below + 397. But if 71°/$_0$ occur below + 200 and 83°/$_0$ below + 397, then 12°/$_0$ must occur between + 200 and + 397; in other words, the most probable percentage of limina falling into the interval between + 200 and + 397 is 12°/$_0$. By extending the same reckoning process to the whole series of observed data, we get a complete distribution of the percentages of the upper limina[3]; these will be found in the 2nd column of Table II.

TABLE II. *Distribution of upper limina* (*deduced from Table I.*).

Between the limits	p = percentage of limina	f = measure of average relative frequency	No. of column in graph
− 694 and − 538	0	0	
− 538 ,, − 370	11	12·9	1
− 370 ,, − 195	17	19·1	2
− 195 ,, 0	20	20·2	3
0 ,, + 200	28	22·7	4
+ 200 ,, + 397	12	12·0	5
+ 397 ,, + 603	8	7·6	6
+ 603 ,, + 807	4	3·9	7
+ 807 ,, +1021	3	2·8	8
+1021 ,, +1239	2	1·8	9

If the D's are equidistant, evidently the percentage for each interval may be taken as proportional to the average relative frequency of such limina; the percentage will therefore without further ado serve as a measure of the average relative frequency. But if (as here) the intervals between the D's are appreciably unequal, a correction is required: each percentage must be divided by its interval. And then—to make the results comparable with those in the cases where no such correction is needed—each such quotient may be multiplied by the general average of all the intervals. Thus in our example a few lines above, 12 must first be divided by 197 (the interval being in this case $397-200$); it is then, to facilitate comparison, multiplied by the average interval, $\frac{1239-(-438)}{9}=197\cdot4$. In this way we get the values in the 3rd column of Table II.

---

[1] In this paper, D_k will be termed less than D_{k+1} whenever $D_k - D_{k+1}$ is negative.

[2] Obviously, the limen is some value greater than D whenever the observer answers either *l* or even *u*.

[3] Any *minus* percentage shows that the experimental series was too short, or that some illegitimate influence was present and affected the D's unequally.

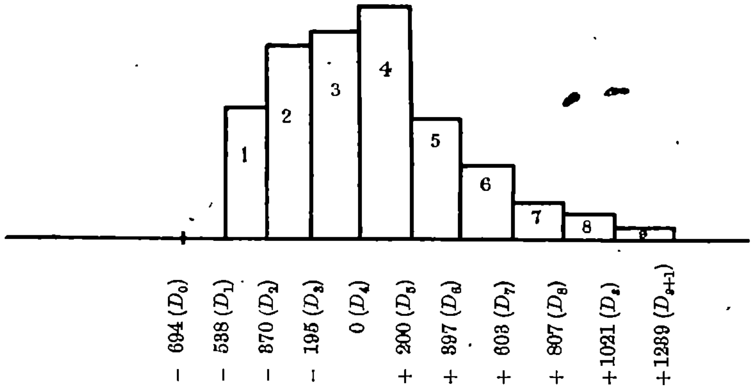


Fig. 1. Actual distribution of Limina.

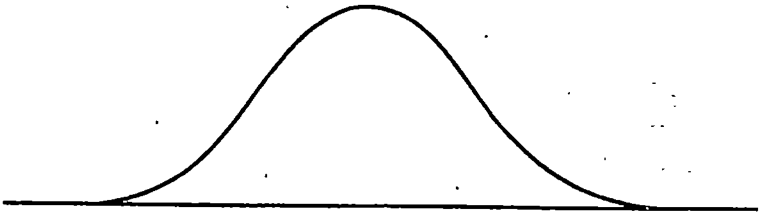


Fig. 2. Distribution calculated from the same data as fig. 1, but by means of Gauss's formula.

Having thus easily arrived at a complete list of average relative frequencies, the next question is how to represent these data summarily. In the opinion of the present writer, there is one and only one procedure, by which any given series of values can be summarized clearly, completely, and without questionable assumptions: this is by means of a *graph*. Once enlightened generally by such a lucid picture of the entire state of affairs, the researcher is in a position to choose any specially interesting characteristics for exact measurement. When he comes to make this particular measurement—and not before—the graphic procedure ought to be replaced by that of analysis.

For this graph, we will take the *D*'s as abscissas and the *f*'s as ordinates. In this way we get figure 1 (the numbering of the *D*'s will be explained later on). Clearly, it differs from the 'normal' distribution (fig. 2) to a remarkable extent. That the asymmetry is no mere chance, is proved at once by its magnitude, its regularity, and by a quite similar shape resulting from Merkel's parallel series, where *S* followed *C*. It hardly needs saying, that the study of such abnormal distributions is likely to be very fruitful for psychology.

The chief point for the present is that our proposed method has brought this abnormality so clearly to view.

3. *First method of measuring the average limen.*

Having obtained a general survey of the distribution of the limina, we come to our second task, the exact measurement of the chief particular characteristics. Foremost, but still only one of several, of these is the mean limen. And even as regards this mean, there are various different values to be considered. First, there is the 'mode' or limen occurring most frequently[1]; it is represented by the highest point in the frequency distribution and evidently lies somewhere between 0 and + 200 (see fig. 1); its position could be found definitely by interpolation, but this is unfortunately a precarious proceeding, depending upon only a small part of the experimental data and upon a more or less arbitrary method of calculation. Secondly, there is the 'median' or the value having just as many limina above as below it; this seems to be the only sort of limen measured previously (without assuming the Gaussian formulae); but again recourse must be had to a precarious interpolation[2]. And lastly, there is the 'average,' or the sum of the liminal values divided by the number of them; of the three means this will be found much the most accurate. In the present paper, the calculation of this one only will be discussed.

For this purpose, let us turn to fig. 1 again. We see that the total distribution is made up of the part distributions belonging to the separate columns respectively (here, nine); the area of each column represents the number of limina between the two magnitudes denoted by the extremities of the column's base. Therefore, using a known rule, the required total average magnitude of all the individual limina is equal to the average of the part averages for the separate columns; 'weight' must of course be allowed for the columns' varying areas. But the part average belonging to any single column is represented with fair approximation by the centre of that column's base; strictly speaking, it must lie somewhat more towards the highest column, but the errors thus engendered in the columns on the left are approximately compensated by errors of similar amount and opposite direction in the columns on the right. Now, let

---

[1] Conceivably, of course, the mode may assume an extreme value instead of a mean one. But it does this so extremely rarely and it presents so many analogies with the true means, that it may conveniently be classed with them.

[2] G. E. Müller, to whom the idea of measuring the mode and the median without assuming the Gaussian formulae is originally due, points out the necessity and the disadvantages of using interpolation (*loc. cit.* S. 42). Formulae for interpolating are given by Lehmann, *Archiv f. d. g. Psychologie*, Bd. VIII. S. 484 ff.

c_k = the centre of the base of any, say the kth, column from the left; if we call the two extremities of this base D_k and D_{k+1}, then $c_k = \frac{1}{2}(D_k + D_{k+1})$*;

p_k = the area of the kth column (2nd column, Table II.);

Σcp denote the sum obtained by multiplying c by p for each column and adding together all the products;

Σp denote the sum of all the p's (= the number of limina);

and L denote the required average limen.

Then $$L = \frac{\Sigma cp}{\Sigma p} = \frac{\Sigma cp}{100} \quad \text{.........................} \quad (a).$$

The meaning of this will be found very simple on working it out, as shown in the following table.

TABLE III. *Calculation of the average upper limen. First method.*

Col.	c			p	cp	
		+	−		+	−
1	$\frac{1}{2}(D_1+D_2) = \frac{1}{2}(-538 - 370) =$		454	11		4994
2	$\frac{1}{2}(D_2+D_3) = \frac{1}{2}(-370 - 195) =$		282·5	17		4802·5
3	$\frac{1}{2}(D_3+D_4) = \frac{1}{2}(-195 + 0) =$		97·5	20		1950
4	$\frac{1}{2}(D_4+D_5) = \frac{1}{2}(0 + 200) =$	100		23	2300	
5	$\frac{1}{2}(D_5+D_6) = \frac{1}{2}(200 + 397) =$	298·5		12	3582	
6	$\frac{1}{2}(D_6+D_7) = \frac{1}{2}(397 + 603) =$	500		8	4000	
7	$\frac{1}{2}(D_7+D_8) = \frac{1}{2}(603 + 807) =$	705		4	2820	
8	$\frac{1}{2}(D_8+D_9) = \frac{1}{2}(807 + 1021) =$	914		3	2742	
9	$\frac{1}{2}(D_9+D_{10}) = \frac{1}{2}(1021 + 1239) =$	1130		2	2260	
				$100 = \Sigma p$	17704	11746·5
					11746·5	
					$5957{\cdot}5 = \Sigma cp$ †	

---

* The numbering of the D's in fig. 1 is taken from this definition.

† The largeness of these numbers is, of course, not due to the present method, but to the unusually large numbers in Merkel's D's (see Table I.).

Hence, the average limen,

$$L = \frac{\Sigma cp}{100} = + \frac{5957 \cdot 5}{100} = +60, \text{ rounded off.}$$

4. *The case of equal intervals.*

Much trouble is saved when, as will usually be the case, all the D's are equidistant. Then the f's, the ordinates for the graph, become identical with the p's. Also our formula (a) can be modified so as to make the calculation much shorter.

To measure the upper limen,

let i = the constant interval between the D's,

D_{s+1} = the smallest D furnishing no u's or l's (D_s = the D next smallest, etc.),

Σg = the sum of the g's for all D's smaller than D_{s+1},

n = the number of judgments for each D,

and L_{up} = the required average upper limen.

Then $$L_{up} = \tfrac{1}{2}(D_{s+1} + D_s) - \frac{i \,.\, \Sigma g}{n} \ldots\ldots\ldots\ldots\ldots\ldots (b)^1.$$

Thus in our previous example, *if* all the intervals had been equal to the average of the intervals in question (*i.e.* those between D_1 and D_{s+1}), this common interval would have been 197·4, while $\Sigma g = 625$ (Table I.); consequently,

$$L_{up} = \tfrac{1}{2}(1239 + 1021) - \frac{197 \cdot 4 \times 625}{100} = -104.$$

The large discrepancy of this from the previous result (+ 60) shows that formula (b) cannot be used unless the D's really have a common interval. We may not substitute the average interval, even when, as here, the differences between the intervals is slight (except in the particular case, that these differences can be shown to tend to compensate one another).

For a lower limen, it will be found similarly that

$$L_{lo} = \tfrac{1}{2}({}_{t+1}D + {}_tD) + \frac{i \,.\, \Sigma 1}{n} \ldots\ldots\ldots\ldots\ldots\ldots (c),$$

where ${}_{t+1}D$ = the largest D furnishing no u's or g's (${}_tD$ = the D next largest, etc.),

---

[1] For proof, see Appendix 1.

Σl = the sum of the l's for all D's larger than ${}_{t+1}D$,

i and n = the same as before,

and L_{lo} = the required average lower limen.

It may be remarked that

$$L_{up} - L_{lo} \text{ will be found equal to } \frac{i \,.\, \Sigma u}{n},$$ [1]

which latter expression is that given by Müller as measuring the 'ideal region of u-judgments.'[2] In fact, the present paper may be regarded generally as an attempt to develop some ideas of Müller a little further[3].

5. *Second and more accurate method of measuring the average limen.*

With but little extra trouble, we can substitute for formula (a) the following more accurate one:

$$L' = \frac{\Sigma ap}{\Sigma p} = \frac{\Sigma ap}{100} \quad\ldots\ldots\ldots\ldots\ldots\ldots(d),$$

where $a_k = D_k + \dfrac{D_{k+1} - D_k}{3} \cdot \dfrac{2f_{k+1} + 3f_k + f_{k-1}}{f_{k+1} + 2f_k + f_{k-1}}$ and is a more approximate value of the average limen for the kth column[4],

f_k = the average relative frequency for the kth column (see Table II., 3rd column),

p_k is as before,

Σ again denotes summation over all values of k,

and L' = the required more approximate limen.

Applying this formula to the same example as before, we get, for instance:

$$\begin{aligned} a_1 &= D_1 + \frac{D_2 - D_1}{3} \cdot \frac{2f_2 + 3f_1 + f_0}{f_2 + 2f_1 + f_0}, \\ &= -538 + \frac{-370 - (-538)}{3} \cdot \frac{(2 \times 19) + (3 \times 13) + 0}{19 + (2 \times 13) + 0}, \\ &= -442, \end{aligned}$$

---

[1] For proof, see Appendix 2.

[2] *Die Gesichtspunkte und Tatsachen der psychophysischen Methodik*, S. 148.

[3] Anyone acquainted with the *Gesichtspunkte etc.* will readily see this for himself. Prof. Müller has, moreover, had the kindness to look critically through the MS. of this paper.

[4] For proof, see Appendix 3.

$$a_2 = D_2 + \frac{D_3 - D_2}{3} \cdot \frac{2f_3 + 3f_2 + f_0}{f_3 + 2f_2 + f_0},$$

$$= -370 + \frac{-195 - (-370)}{3} \cdot \frac{(2 \times 20) + (3 \times 19) + 13}{20 + (2 \times 19) + 13},$$

$$= -280.$$

Having in this manner calculated all the values of a, we can construct a table as before.

TABLE IV. *Calculation of the average upper limen. Second method.*

Col.	a +	a −	p	ap +	ap −
1		442	11		4862
2		280	17		4760
3		96	20		1920
4	97		23	2231	
5	289		12	3468	
6	490		8	3920	
7	696		4	2784	
8	907		3	2721	
9	1114		2	2228	
			100	17352	11542
				11542	
				$5810 = \Sigma ap$	

Hence $$L' = \frac{\Sigma ap}{100} = \frac{5810}{100} = 58{\cdot}1.$$

Thus, although the distribution investigated is very asymmetrical, and although the value for each column by this method differs sensibly from that by the former method (see Table III.), yet the total results by the two methods very closely coincide with one another. Still, the additional work involved in the second method is so insignificant in comparison with the labour of originally making the whole experimental series, that it seems worth undertaking even for such a slight gain of accuracy.

6. *Mean deviation.*

Next to the mean, the most important characteristic of the distribution of the limina is the mean deviation from the mean. By our method we are able to measure the upper and lower mean deviations separately. Here the advantage of the a (used in the 2nd method) over the c (used in the 1st method) is much more marked than before, as the errors of c on the right and left sides respectively of the distribution do not compensate one another, but all tend in the same direction, namely, to make the calculated value too big.

The average deviation from the average will be given by the formula

$$\theta = \frac{\Sigma pd}{\Sigma p} \quad \text{.............................} (e),$$

where $d_k = | a_k - L' |$[1],

p_k = the same as before,

Σ indicates summation over all values of k,

and θ is the required average deviation.

The only difficulty concerns the particular column into which the average limen itself falls. This column must evidently furnish two p's and two d's, one on each side of L'. Very fair approximations to these values are given by the following formulae:

$$p_r = p_a \frac{D_r - L'}{D_r - D_l} \quad \text{.............................} (f),$$

where D_r and D_l are the D's on the right and left respectively of L', p_a is the percentage for the whole column in which L' falls, and p_r is the required percentage for the portion of this column on the right of L'. To find p_l, we have similarly

$$p_l = p_a \cdot \frac{L' - D_l}{D_r - D_l} \quad \text{.............................} (g).$$

Further,

$$a_r = \frac{D_r - L'}{2} \quad \text{.............................} (h),$$

where D_r means the same as before and a_r is the required a on the right of L'. And similarly,

$$a_l = \frac{L' - D_l}{2} \quad \text{.............................} (i).$$

We get, then, the following table.

TABLE V. *Average lower deviation (from average upper limen).*

Col.	$d\ (= 58{\cdot}10 - a)$	p	pd
1	500·10	11	5501
2	338·10	17	5748
3	154·10	20	3082
4	29·25	6·7 $(= p_l)$	194
		54·7 $(= \Sigma p)$	14425 $(= \Sigma pd)$

Hence

$$\theta^{lo} = \frac{\Sigma pd}{\Sigma p} = \frac{14425}{54{\cdot}7} = 261.$$

---

[1] The brackets denote that this difference is always treated as positive. L' was found in the preceding section, see equation (d).

For the average upper deviation we get similarly:

TABLE VI. *Average upper deviation (from average upper limen).*

Col.	$d\ (=a-58{\cdot}10)$	p	pd
4	59·45	16·3 $(=p_r)$	969
5	280·90	12	2771
6	431·90	8	3455
7	637·90	4	2552
8	848·90	3	2547
9	1055·90	2	2112
		45·3 $(=\Sigma p)$	14406 $(=\Sigma pd)$

Hence $$\theta^{up}=\frac{\Sigma pd}{\Sigma p}=\frac{14405}{45{\cdot}3}=319.$$

We can further determine the 'measure of precision' h from the usual equation, $h=\frac{1}{\theta\sqrt{\pi}}$. And actually I have found this value of h to coincide very closely with that obtained by means of Müller's method. For instance, we find in our above example the average deviation (upper and lower) $=\theta=288{\cdot}31$. As $\sqrt{\pi}=1{\cdot}772$, we get

$$h=\frac{1}{288{\cdot}31\times1{\cdot}772}=0{\cdot}00196,$$

whereas Müller, using a unit of measurement 10 times larger than ours, gives $h_{oI}=0{\cdot}0180$. The coincidence becomes still closer when the distribution of values is more approximately Gaussian, as occurs for instance in Merkel's lower limen with C preceding S.

Here our method gives

$$h=\frac{1}{274{\cdot}781\times1{\cdot}772}=0{\cdot}00205,$$

whereas Müller's h_{uII} is 0·020 (*loc. cit.* p. 71).

7. *Conclusion.*

To sum up, the advantages claimed by the proposed method are:

(1) It dispenses with all questionable assumptions (as of Gauss's law).

(2) It utilizes the experimental data exhaustively. Hereby the exactness and, above all, the fulness of the results are greatly increased.

(3) Nevertheless, the calculations are less difficult than those required for the best methods now in use.

APPENDIX.

(1) Proof that

$$\frac{\Sigma cp}{100} = \tfrac{1}{2}(D_{s+1} + D_s) - i/n \,.\, \Sigma g.$$

By definition of c,

$$2\Sigma cp = p_s(D_{s+1} + D_s) + p_{s-1}(D_s + D_{s-1}) + \ldots + p_2(D_3 + D_2) + p_1(D_2 + D_1),$$

and therefore, by definition of p,

$$= \frac{100}{n}\left[(n - g_s)(D_{s+1} + D_s) + (g_s - g_{s-1})(D_s + D_{s-1}) + \ldots \right.$$
$$\left. + (g_3 - g_2)(D_3 + D_2) + g_2(D_2 + D_1)\right],$$
$$= 100(D_{s+1} + D_s) - \frac{100}{n}\left[g_s(D_{s+1} - D_{s-1}) + g_{s-1}(D_s - D_{s-2}) + \ldots \right.$$
$$\left. + g_2(D_3 - D_1)\right],$$
$$= 100(D_{s+1} + D_s) - \frac{100}{n} \,.\, 2i\Sigma g,$$

from which $$\frac{\Sigma cp}{100} = \tfrac{1}{2}(D_{s+1} + D_s) - i/n \,.\, \Sigma g.$$

(2) Proof that $L'_{up} - L'_{lo} = i/n \,.\, \Sigma u.$

Let N denote the number of intervals between D_{p+1} and ${}_tD$,

= the number of D's producing a mixture of different judgments,

$= 1/n \,.\, (\Sigma g + \Sigma u + \Sigma 1).$

Then $$D_{s+1} + D_s - ({}_{t+1}D + {}_tD) = 2(D_{s+1} - {}_tD) = 2iN.$$

Consequently,

$$L'_{up} - L'_{lo} = \tfrac{1}{2}(D_{s+1} + D_s) - i/n \,.\, \Sigma g - \left[\tfrac{1}{2}({}_{t+1}D + {}_tD) + i/n \,.\, \Sigma 1\right],$$
$$= iN - i/n \,.\, (\Sigma g + \Sigma l),$$
$$= i/n \,.\, \Sigma u.$$

(3) Proof of the formula:

$$a_k = D_k + \frac{D_{k+1} - D_k}{3} \,.\, \frac{2f_{k+1} + 3f_k + f_{k-1}}{f_{k+1} + 2f_k + f_{k-1}}.$$

The vertical and horizontal straight lines of fig. 3 have the same meaning as those of fig. 1; that is, the height of each column represents the *observed average* relative frequency (f in Table II.) of all the limina falling between the two D's at the extremities of the column's base. The curved line, on the other hand, represents the *unknown particular* relative frequency of the limina for every particular value of D; it is thus only limited by the equations: area α = area β, area γ = area δ, area ϵ = area ζ, etc.

Our problem is to find an approximation to the average size of all the limina falling between D_k and D_{k+1}. The true average size is evidently represented by the abscissa of the centroid of the area bounded by the ordinates f_k and f_{k+1}, the axis of x, and the curve. And a very fair approximation will be seen to be furnished by the abscissa of the centroid of the area bounded by the ordinates f_k and f_{k+1}, the axis of x, and the straight line $y_k y_{k+1}$, where $y_k = \frac{1}{2}(f_{k-1} + f_k)$ and $y_{k+1} = \frac{1}{2}(f_k + f_{k+1})$. This approximative value will, it is true, always lie slightly too near the ordinate on the concave side of the curve (in fig. 3, too near to D_k). But all such errors must almost exactly compensate one another, not only as regards the ascending compared with the descending branch of the curve, but even as regards the upper compared with the lower portion of each branch separately.

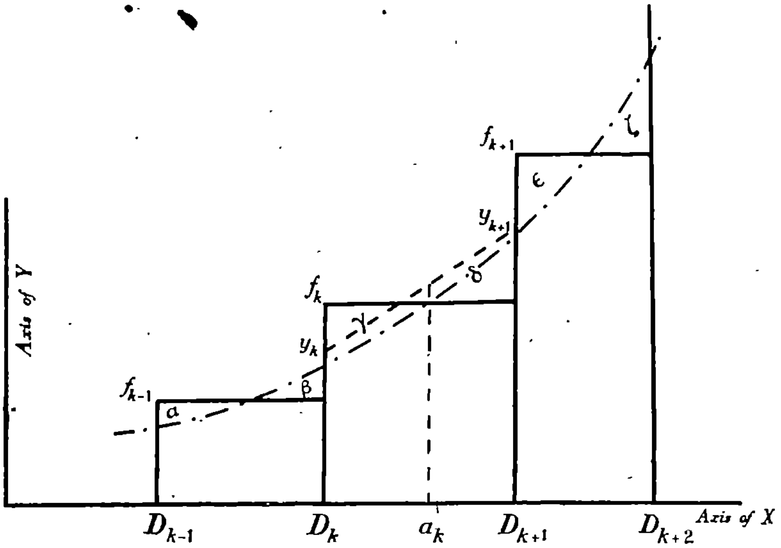


FIG. 3.

Now let our approximative abscissa be denoted as before by a_k. We get, by known rule, the equation

$$a_k = D_k + \frac{\int_0^\Delta xy \,.\, dx}{\int_0^\Delta y \,.\, dx},$$

where the equation to the straight line $y_k y_{k+1}$ is

$$y = \frac{x\,(y_{k+1} - y_k)}{D_{k+1} - D_k} + y_k,$$

and
$$\Delta = D_{k+1} - D_k.$$

Hence, on eliminating the variable y,

$$a_k = D_k + \frac{\int_0^\Delta 1/\Delta \,.\, x^2 y_{k+1} \,.\, dx - \int_0^\Delta 1/\Delta \,.\, x^2 y_k \,.\, dx + \int_0^\Delta x y_k \,.\, dx}{\int_0^\Delta 1/\Delta \,.\, x y_{k+1} \,.\, dx - \int_0^\Delta 1/\Delta \,.\, x y_k \,.\, dx + \int_0^\Delta y_k \,.\, dx}$$

$$= D_k + \frac{\Delta^2 \left(\tfrac{1}{3} y_{k+1} - \tfrac{1}{3} y_k + \tfrac{1}{2} y_k\right)}{\Delta \left(\tfrac{1}{2} y_{k+1} - \tfrac{1}{2} y_k + y_k\right)},$$

or, on replacing y_k, y_{k+1}, and Δ by values of f and D, and then simplifying,

$$= D_k + \frac{D_{k+1} - D_k}{3} \,.\, \frac{2f_{k+1} + 3f_k + f_{k-1}}{f_{k+1} + 2f_k + f_{k-1}},$$

which was required to be proved[1].

---

[1] When the D's are equidistant, there is another still closer approximation given by a_k', where

$$a_k' = D_k + \frac{i}{6} \,.\, \frac{5f_{k+1} + 18f_k + f_{k-1}}{f_{k+1} + 6f_k + f_{k-1}};$$

but the gain by this way is not sensible practically, and the formula becomes very complicated when the intervals are not equal.

THE ILLUSION OF COMPARED HORIZONTAL AND VERTICAL LINES.

BY G. DAWES HICKS AND W. H. R. RIVERS.

Investigation of the rôle of movement factors in the production of the illusion by the comparison of momentary and prolonged exposure. Description of apparatus. Method of right and wrong cases. The illusion uninfluenced by duration of exposure. Distinct answers more numerous with momentary exposure. Accessory observations. General conclusion.

THE experiments, of which an account is given in this article, were undertaken in the hope of obtaining an answer to the question whether sensations arising from movements of the eyes play any part in the production of the illusion which arises when the length of a vertical is compared with that of a horizontal line. This question is only one of many that centre round the general problem of the influence of movement factors upon visual spatial perception and localisation in space.

So far as we know, Oppel was the first to call attention, in 1854, to the circumstance that there is a tendency to over-estimate the length of vertical lines relatively to horizontal lines. The erroneous estimation has usually been explained as due to the greater difficulty of making vertical eye movements, and by some the greater difficulty has been connected with the fact that upward and downward movements of the eye require the activity of more than a single pair of muscles. Wundt, for example, lays emphasis upon the circumstance that, in the upward and downward movement, the rectus superior and inferior and the obliqui are together involved, and that whilst the inferior obliquus assists the superior rectus to raise the eye, and the superior obliquus assists the inferior rectus to lower the eye, yet each of these is antagonistic to the other as regards tension. Accordingly he infers that upward and downward movements require greater muscular effort than do movements outwards and inwards, and, assuming that muscular effort furnishes a criterion of the movement in each case, we have, he thinks, a natural explanation of the different estimation of the lengths of vertical and horizontal lines[1]. Other reasons for the phenomenon in question have, it is true, been offered. Külpe, whilst not rejecting the influence of movement factors, refers to the fact that the visual field, whether monocular or binocular, and whether for the resting or moving eye, has a less extension in the vertical than in the horizontal direction[2]. Lipps, on the other hand, rejects *in toto* the influence of movement factors, and regards the illusion as an error not of perception but of judgment. A horizontal line is, in his view, under-estimated because of the tendency, the presence of which he believes he has established, to conceive lateral figures as compressed by their ends, whereas a vertical line suggests a striving upwards, characteristic of life[3]. It must be admitted, however, that these theories, especially the last mentioned, are vague and unsatisfactory. At present, the only clear and definite psychological explanation in the field is that which accounts for the illusion by the hypothesis of movement factors, although such movements may not be specifically allocated after the manner of Wundt to particular muscles.

In order, then, to test the validity of the prevalently accepted theory, a series of experiments was devised and carried out, during the session 1903–4 and following sessions, in the Cambridge Psychological Laboratory. These experiments consisted in studying, by means of the method of right and wrong cases, the way in which the illusion was affected by the duration of exposure. A series of momentary exposures of the figure, short enough to exclude the possibility of any such eye movements as would be necessary if estimation of length were dependent upon them, and a like series of prolonged exposures were given, and after each separate exposure the observer recorded his judgment, which was taken down by the experimenter.

The method employed was to exhibit a vertical and a horizontal line at right angles to one another. The horizontal line was to the right of, and below the point of junction with, the vertical line, so that the figure resembled the letter L. The length of either line could be varied at will. The figure was projected on a screen by means of the apparatus shown in Fig. 1[1]. The rays of light from an incandescent gas-lamp, leaving the condenser *A*, pass through a rectangular slit in the screen *B* shown on a larger scale in Fig. 2. The whole aperture in the screen is in the form of a cross of which any part or the whole of each limb can be closed by small shutters which slide in grooves. The sides of the grooves are graduated so that the length of each limb can be recorded. In Fig. 2 the slit is shown as it was used in our observations, an inverted image being thrown on the screen. The angular beam of light from the slit passes through an aperture in the pendulum *C* and then by means of the lens *E* an image of the figure is thrown on

---

[1] *Grundzüge*, 4te Aufl., Bd. ii. S. 137 sqq.

[2] *Grundriss der Psychologie*, S. 379.

[3] Art. on "Aesthetische Factoren der Raumanschauung" in *Beiträge* published as *Festgruss* to Helmholtz, Hamburg and Leipzig, 1891. See also *Raumaesthetik und geometrisch-optische Täuschungen*, by Lipps, Leipzig, 1897.

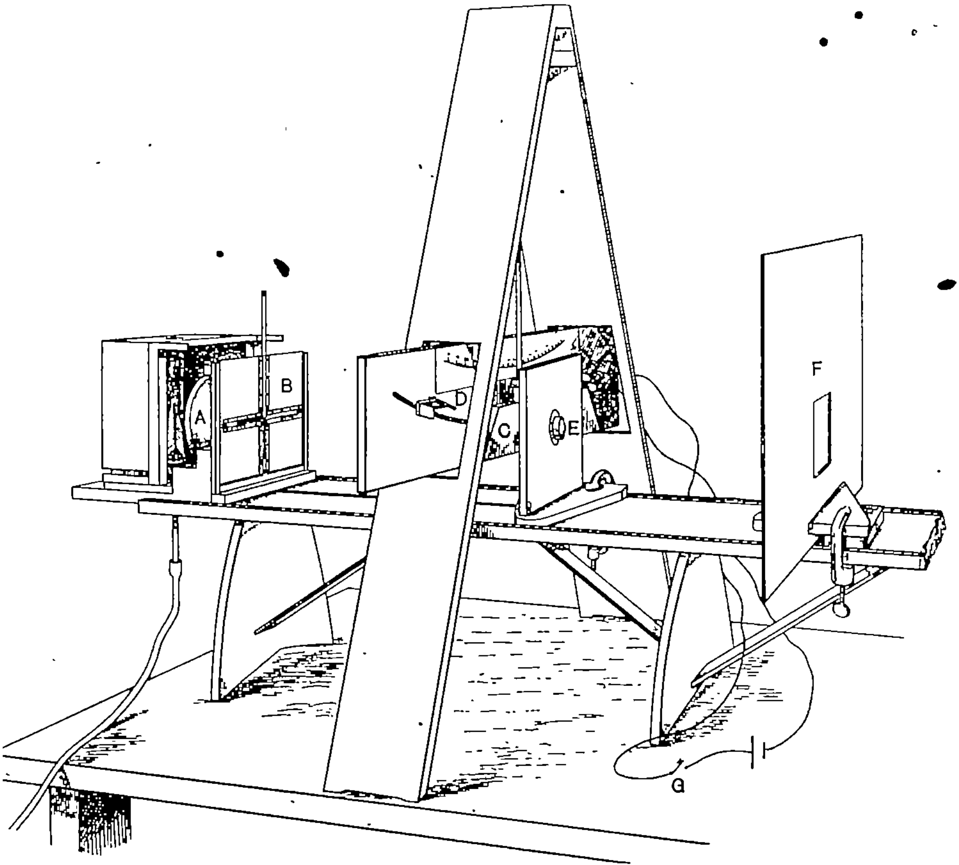


FIG. 1.

---

[1] This apparatus was devised by Mr F. N. Hales.

a translucent glass plate in the screen *F*. When no obstacle is interposed an observer sees on the screen *F* an image which has half the dimensions of the slit in *B*. In order to obtain brief exposures, the pendulum *C* is placed so that it is held by the magnet, being released when the key *G* is opened and caught at the end of its swing by the catch *D*. The time of exposure can be regulated by varying the size of the aperture in the pendulum and the distance through which the pendulum swings. In the observations we call 'momentary' throughout this paper the exposure was less than $\frac{1}{60}$th of a second and was probably short enough to exclude all movements of the eyes during the exposure, and was certainly sufficient to exclude such movements along the two lines of the figure as would be necessary on the supposition that estimation of their length is based on muscular sensations.

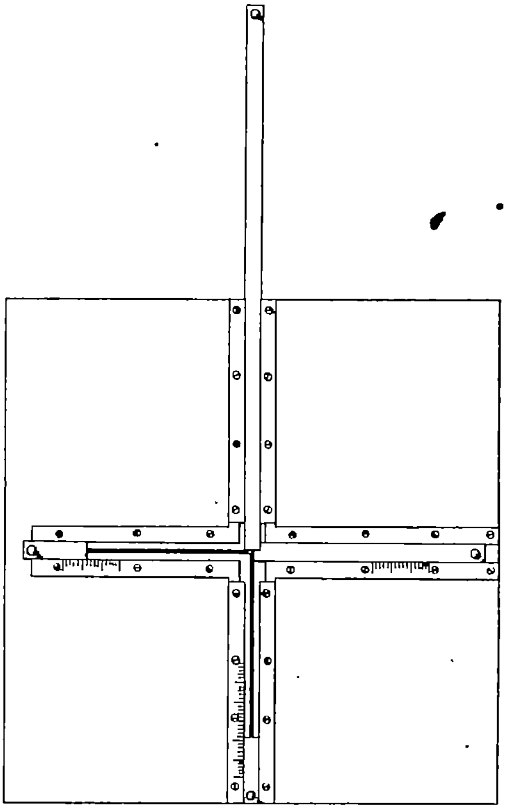


FIG. 2.

The special advantage of the apparatus is that the appearance and disappearance of the figure are practically instantaneous. If the pendulum is moved slowly, it is found that a very small movement brings the whole figure into view. When the pendulum is hanging, any alteration which is being made in the slit can be hidden from the observer by means of a screen at the back of the pendulum which is not shown in the figure. In our observations the screen was situated 65 cm. from the observer whose head was placed on a chin-rest.

We made observations both with constant horizontal, varying the length of the vertical, and with constant vertical, varying the length of the horizontal. After some preliminary observations directed to ascertain the approximate amount of the illusion in each subject, a series of observations lasting for six days was made with the horizontal as the standard. In this series we used no fixation-mark on the screen *F* but in a second series a small fixation-mark was used in the position of the point of junction of the two lines. In the second series the horizontal was also the standard and in a third series the vertical became the standard while the horizontal was varied. Each series occupied six days, the momentary and prolonged exposures being given first on alternate days. In these experiments we were ourselves the subjects, one recording the observations made by the other, and both of us knew which of the two lines was the standard. It was found that this knowledge introduced a very unsatisfactory feature into the experiments as each observer noted an unmistakable tendency to compare one line, horizontal or vertical, not with the simultaneously presented line but with the corresponding line seen in a previous exposure. In order to eliminate the effects of this tendency and other possible results of knowledge on the part of the observers, we arranged a further series of experiments, an account of which will be found below. It may be well, however, in the first instance to give the results of our preliminary investigations.

In the following tables Series I gives the results of the observations with the horizontal standard in which no fixation-mark was used; Series II gives the results with the horizontal standard when a fixation-mark was used; and Series III the results with the vertical as standard, the fixation-mark being retained. In Series I and II the observer was asked to say whether the vertical line was "distinctly longer," "longer," "same or longer," "same or shorter," "shorter," or "distinctly shorter," than the horizontal, or whether there was "no difference" between them. In Series III he was asked to judge in same manner of the horizontal line relatively to the vertical.

TABLE I.

Subject W. H. R. R. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES I.	37·0	19	3	1	25	1	0
	37·5	19	6	2	21	4	1
	38·0	14	9	6	15	11	1
	38·5	13	6	12	7	11	10
	39·0	1	4	15	4	7	11
	39·5	2	4	20	2	3	24
	40·0	0	2	26	0	2	29
	Total	68	34	82	74	39	76
SERIES II.	37·0	28	1	0	29	0	0
	37·5	24	1	3	23	6	0
	38·0	17	7	5	18	11	0
	38·5	11	7	11	1	9	19
	39·0	7	9	13	4	6	19
	39·5	3	4	22	0	3	26
	40·0	2	1	26	0	0	29
	Total	92	30	80	75	35	93

TABLE II.

Subject W. H. R. R. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES III.	38·0	27	2	1	28	2	0
	38·5	26	3	1	23	6	1
	39·0	23	5	2	16	11	3
	39·5	18	3	9	4	11	15
	40·0	13	6	11	1	5	24
	40·5	3	8	19	0	2	28
	41·0	2	4	23	0	0	30
	Total	112	31	66	72	37	101

TABLE III.

Subject G. D. H. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES I.	35·5	17	4	9	30	0	0
	36·0	18	4	8	20	8	2
	36·5	6	8	16	7	18	5
	37·0	9	6	15	0	9	21
	37·5	5	1	24	1	2	27
	38·0	2	2	26	0	0	30
	Total	57	25	98	58	37	85
SERIES II.	35·5	25	2	3	26	4	0
	36·0	22	3	5	24	6	0
	36·5	13	8	9	4	24	2
	37·0	8	5	17	1	8	21
	37·5	2	8	20	0	2	28
	38·0	2	7	21	0	0	30
	Total	72	33	75	55	44	81

TABLE IV.

Subject G. D. H. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES III.	42·5	27	3	0	30	0	0
	43·0	29	1	0	30	0	0
	43·5	17	6	7	20	9	1
	44·0	8	9	13	7	16	7
	44·5	2	6	22	2	18	10
	45·0	1	6	23	0	1	29
	Total	84	31	65	89	44	47

As will be seen from the above tables, the general result obtained from these experiments was that the illusion appeared to be of almost exactly the same amount when the lines were exposed momentarily as when the exposure was prolonged. In Series I, the amount of the illusion is exactly the same in both cases for observer R., whilst for observer H. it is slightly greater in the case of the prolonged; in Series II for observer R. it is slightly greater in the case of the momentary, whilst for observer H. it is again slightly greater in the case of the prolonged; in Series III for observer R. the illusion has practically vanished in both cases, whilst for observer H. it is slightly greater once more in the case of the prolonged. Table V, which gives the lengths of the lines that correspond most nearly to a judgment of equality on the part of the two observers[1], enables the amount of the illusion in each case to be seen at a glance.

TABLE V.

Standard horizontal 40 mm.; standard vertical 40 mm.

	I.		II.		III.	
	M.	P.	M.	P.	M.	P.
W. H. R. R. ...	38·5	38·5	38·5	38·25	40·0	39·25
G. D. H.	36·25	36·5	36·75	36·5	43·75	44·0

After these preliminary investigations, we planned, as already stated, a further series of experiments, so arranged that the effects of the tendency to which allusion has been made of comparing the varying line with the corresponding line of the previous exposure were eliminated. The further series was designed to extend in each case over a period of fifteen days, and observations were made with five different persons as subjects. In these observations, whilst one of the two lines given for comparison was always either the horizontal or vertical standard, yet neither was given in any regular order. Thus, for example, in the case of the subjects R. and H., the experimenter threw successively upon the screen members of Series II and III taken promiscuously, and the observer was totally unaware in regard to any one exposure as to which of the two lines was the standard. He was asked simply to name the line which seemed to him the "longer" (recording his judgment in the form vertical, or horizontal, "longer," or "distinctly longer," or "same or longer"), or to say when he could discern "no difference" between the two lines. Series IV, therefore, consisted of the members of Series II and III mixed together indiscriminately. For the sake of convenience, however, in the subjoined tables, the results with the two standards have been sorted out, and those with the horizontal standard are called Series IV A, those with the vertical standard IV B. In order to compare the results with the two standards, it has been necessary to convert the answers into the form they would have taken if the reference had been in each case (as in Series II and III) to the variable line. Thus, if with the horizontal standard the horizontal was said to be "longer," we have converted that into an answer "vertical shorter," and, similarly, if with the vertical standard the vertical was said to be "longer," we have converted that into an answer "horizontal shorter." There was, of course, a fixation-mark as in Series II and III. In the case of each of the three new subjects, some preliminary observations were required to ascertain the approximate amount of the illusion.

---

[1] In reference to this table, cp. the description of Table xv below, p. 253.

TABLE VI.

Subject W. H. R. R. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV A.	37·0	29	1	0	30	0	0
	37·5	29	1	0	30	0	0
	38·0	27	2	1	29	1	0
	38·5	23	3	4	26	4	0
	39·0	16	4	10	24	6	0
	39·5	8	11	11	4	10	16
	40·0	4	12	14	1	4	25
	Total	136	34	40	144	25	41

TABLE VII.

Subject W. H. R. R. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV B.	38·0	29	1	0	30	0	0
	38·5	28	2	0	30	0	0
	39·0	26	3	1	29	1	0
	39·5	19	8	3	27	3	0
	40·0	15	7	8	27	3	0
	40·5	13	4	13	11	7	12
	41·0	6	5	19	3	7	20
	Total	136	30	44	157	21	32

TABLE VIII.

Subject G. D. H. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV A.	35·5	28	2	0	29	1	0
	36·0	25	2	3	24	6	0
	36·5	14	5	11	20	5	5
	37·0	3	2	25	11	9	10
	37·5	6	1	23	1	9	20
	38·0	3	1	26	0	1	29
	Total	79	13	88	85	31	64

TABLE IX.

Subject G. D. H. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV B.	42·5	19	3	8	26	3	1
	43·0	16	1	13	23	4	3
	43·5	11	3	16	13	9	8
	44·0	3	0	27	3	9	18
	44·5	0	2	28	1	4	25
	45·0	0	1	29	0	0	30
	Total	49	10	121	66	29	85

TABLE X.

Subject E. B. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV A.	39·0	20	8	2	20	10	0
	39·5	6	18	6	11	18	1
	40·0	6	8	16	3	14	13
	40·5	2	5	23	1	16	13
	41·0	0	6	24	0	5	25
	Total	34	45	71	35	63	52

TABLE XI.

Subject E. B. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV B.	39·0	22	8	0	19	10	1
	39·5	18	11	1	19	10	1
	40·0	14	13	3	12	16	2
	40·5	8	8	14	3	21	6
	41·0	1	10	19	1	16	13
	Total	63	50	37	54	73	23

TABLE XII.

Subject L. J. L. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV A.	38·0	21	6	3	18	5	7
	38·5	22	5	3	16	5	9
	39·0	8	4	18	4	6	20
	39·5	7	2	21	1	0	29
	40·0	3	2	25	0	0	30
	Total	61	19	70	39	16	95

TABLE XIII.

Subject L. J. L. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV B.	40·0	26	3	1	30	0	0
	40·5	20	1	9	26	1	3
	41·0	15	2	13	25	2	3
	41·5	8	4	18	17	5	8
	42·0	10	2	18	1	2	27
	42·5	1	1	28	2	2	26
	Total	80	13	87	101	12	67

TABLE XIV.

Subject C. S. Standard horizontal 40 mm.

	Length of vertical	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV A.	36·0	20	7	3	28	2	0
	36·5	7	6	17	15	12	3
	37·0	12	7	11	15	12	3
	37·5	5	9	16	4	12	14
	38·0	2	4	24	3	9	18
	38·5	1	0	29	0	2	28
	Total	47	33	100	65	49	66

TABLE XV.

Subject C. S. Standard vertical 40 mm.

	Length of horizontal	Momentary			Prolonged		
		shorter	no diff.	longer	shorter	no diff.	longer
SERIES IV B.	41·5	28	1	1	29	1	0
	42·0	26	4	0	24	5	1
	42·5	17	8	5	15	10	5
	43·0	12	12	6	9	13	8
	43·5	7	16	7	6	12	12
	44·0	2	11	17	2	7	21
	Total	92	52	36	85	48	47

The general result of this extensive series of experiments confirms with surprising uniformity the result previously obtained from our preliminary investigations. Here, too, the amount of the illusion was not affected by the differing times of exposure. As will be seen from the above tables, in Series IV A the amount of the illusion for observer R. is exactly the same with both momentary and prolonged exposures (there being in his case scarcely any illusion at all), for observers H., B., and S., it is slightly greater with the momentary, and for observer L. slightly greater with the prolonged; in Series IV B it is for observer R. again exactly the same with both, for observers H., B., and L., slightly greater with the prolonged, and for observer S. slightly greater with the momentary.

The facts just summarised are brought out clearly in Table XVI, in which are given the lengths of the lines that seem, from an analysis of the preceding tables, to correspond most nearly to a judgment of equality on the part of each of the observers. In each case, equality to the standard is taken to be represented by that line concerning which judgments of "longer" and "shorter" are fairly evenly balanced. Thus, for example, on the basis of Table VIII (the momentary exposures), we have assumed that the observer regarded a vertical line of 36·5 mm. to be equal in length to the horizontal line of 40 mm., because, in his case, with a vertical line of that length the number of judgments "longer" and "shorter" were almost equal, namely 14 and 11, whilst 36 mm. was obviously regarded as too short and 37 mm. obviously too long. Inspection of the tables will for the most part leave no doubt whatever as to which of the lines most nearly corresponds to a judgment of equality. Difficulty arises only in the case of L. J. L., whose observations are throughout somewhat less consistent than those of the other subjects. Thus, a cursory examination of Table XIII (the momentary exposures) might lead to the conclusion that the nearest approach to equality was at 41 mm. But with a horizontal line of 42 mm. the judgments "shorter" are still almost as numerous as the judgments "longer." We have, therefore, fixed upon the horizontal line 41·5 mm. as that of equality. Perhaps not the least noteworthy feature exhibited by these experiments is that in the case of nearly all the observers a difference in the exposed lines so small as ½ mm. should occasion such a marked and decided difference in the recorded judgments. Certainly, this is not what one would have anticipated beforehand. The very great accuracy of estimation may be shown in another way which gives an additional guarantee for the trustworthiness of the results. There is an extraordinary degree of correspondence between the results with the two standards. Taking the figures for the momentary exposures as an example, the length of the vertical with the horizontal standard for the observer R., 39·25, would lead one to expect a length of 40·8 for the horizontal with the vertical standard and as a matter of fact it was 40·5. Similarly for the other four observers the figures to be expected were 43·8, 40·5, 41·3, and 43·2 respectively while the actual figures were 43·25, 40·25, 41·25, and 43·5. The correspondence is in every case very close and the greatest divergence is only ·55 mm.

TABLE XVI.

Standard horizontal 40 mm.; standard vertical 40 mm.

	IV A.		IV B.	
	M.	P.	M.	P.
W. H. B. R.	39·25	39·25	40·5	40·5
G. D. H.	36·5	37·0	43·25	43·75
E. B.	39·5	39·75	40·25	40·5
L. J. L.	38·75	38·5	41·25	41·5
C. S.	37·0	37·25	43·5	43·0

So far we have grouped together under the headings "longer" and "shorter" respectively three judgments of different degrees of certainty, and have taken no account of the grading of the judgments. To the latter feature we would now direct attention. In Table XVII we give an analysis of the judgments according to their definiteness and distinctness, ignoring the fact as to whether the varying line was pronounced to be "longer" or "shorter." The first column of this table contains the number of distinct judgments, irrespective of the circumstance whether they were judgments expressing the longer or shorter character of the varying line; in the second column the judgments "longer" and "shorter," which were given without any qualification, are grouped together; in the third column are the totals of the judgments expressed in the form "same or longer," and "same or shorter." The judgments of "no difference" may be regarded as indicating a further degree of uncertainty.

TABLE XVII.

	Momentary				Prolonged			
	Distinct	Definite	Indefinite	No diff.	Distinct	Definite	Indefinite	No diff.
W. H. R. R. ...	55	177	131	57	32	229	113	46
G. D. H.	37	187	113	23	10	127	163	60
E. B.	3	91	111	95	0	57	107	136
L. J. L.	87	101	110	32	77	126	99	28
C. S.	21	184	70	85	5	184	74	97
Total	203	740	535	292	124	723	556	367

The most interesting fact that comes out from a perusal of this table is that the number of distinct judgments is very decidedly greater with the momentary than with the prolonged exposures,—a disproportion that would probably have been still more evident had we enlarged the series and exhibited lines a degree or two longer and shorter than the maximum and minimum lengths actually used[1]. On the other hand, the number of indefinite judgments (including those of "no difference") is decidedly greater with the prolonged than with the momentary exposures. This result agrees entirely with what was found to be observed introspectively. All five observers noticed that at the beginning, before they had grown accustomed to the method of observation, it was considerably easier to form a judgment in regard to the prolonged exposures than in regard to the momentary ones. But after a few sittings greater confidence began to manifest itself in regard to the latter, until ultimately, for at least three of the observers, judgment in regard to the momentary exposures became much more immediate and pronounced than in regard to the prolonged. With the momentary exposures the differences in the lengths of the lines seemed very marked, and when there was uncertainty it appeared to be due to bad fixation of the image on the screen. With the prolonged exposures, on the other hand, it was often extremely difficult to come to a decision, and the uncertainty in this case was clearly due to hesitation, and frequently to oscillation of judgment. Moreover, the hesitation in the case of the prolonged was not lessened by practice, and it made its appearance sometimes even when the difference between the two lines was actually at the maximum. With regard, however, to the momentary, the observer, provided his fixation was good, came through practice to feel more and more confident, and oscillation of judgment scarcely manifested itself at all. For example, subject E. B. reported, in one of his final sittings, that he had felt much more decided in regard to the momentary, even as compared with those cases in which he had given his judgment in regard to the prolonged apparently without hesitation.

---

[1] As a matter of fact, this was done in the case of some of the observers, and, although the figures are not here given, the result fully justifies the above surmise.

Another fact, which connects itself with what has just been said, is here deserving of mention. In Series IV it of course frequently happened that *both* vertical and horizontal lines were different in successive exposures. All the observers noticed, in such cases, an extraordinary change in the apparent size of the image as a whole. Often the figure exhibited seemed at least twice, sometimes more than twice, as large (or small) as that which had immediately preceded it. Although this change was occasionally noticed with the prolonged exposures, it was more frequent and much more pronounced with the momentary. This circumstance does not appear to have had any appreciable effect upon the judgments given, but to the observer himself it was a distracting influence. Subject H. reports that in the case of the larger figures he became conscious afterwards of having had the tendency to give the preference to the horizontal line. The greater definiteness of the answers with the momentary exposures is of great interest. It shows that though the total number of right and wrong answers brings out no difference in the illusion with momentary and prolonged exposures, the grading of the answers points to the illusion being more decided and definite with the former. In the case of at least two of the observers introspective observations were made which point in the same direction. It was noticed that immediately the figure appeared in the prolonged exposures there was a tendency to give a definite answer which was checked by the oscillation to which we have already referred. Whether such early answers, if they had been given, would have been right or wrong, we have no means of telling. But these introspective observations give the clue to the explanation of the greater definiteness with the momentary exposures, for they show that the immediate judgment was checked and rendered uncertain by factors which came into play later. The factors just mentioned may have been the sensations arising from the movements of the eyes which in such case would be rather a disturbing feature than, as is commonly supposed, the actual basis of the illusion. It does not seem to us, however, that this is a plausible explanation; it is much more likely that the disturbing factors in the prolonged exposures are of a more recondite nature. It would seem probable that the immediate answers are based more purely on the sensory data, while the answers with the prolonged exposures are the results of the complication introduced by such factors as knowledge about the nature of the illusion, experience arising from instruction in drawing, etc.

One of us has found[1] that in observations on illusions more definite and consistent results may be obtained from people of low culture than from civilised observers, and he has explained this result by referring the greater uncertainty of the civilised observer to the influence of knowledge of the illusion and other factors of a similar nature. The facts recorded in this paper seem to show that the observations of the civilised observer may become more definite the more such factors can be excluded and afford valuable confirmation of the correctness of the explanation of the greater definiteness and consistency of the savage in observations of this kind.

We have referred above to the way in which a tendency was manifested in the series, where only one standard was used, to compare the varying vertical or the varying horizontal with the vertical or horizontal of the preceding exposure. Each of the two observers became aware that he was liable to let his judgment be influenced in this manner. After Series II had been finished, the following test was tried upon observer R. The longest vertical we had been using (40 mm.) was several times repeated, and midway between the repetitions one exposure of the shortest vertical we had been using (37 mm.) was made. In each of a number of trials the short vertical was immediately judged to be "enormously" shorter. A corresponding result was obtained when a succession of the shortest verticals was thrown upon the screen, with the longest vertical interposed once in the middle; the latter was immediately judged to be "enormously" longer. Even when a succession of exposures of the medium vertical (38·5 mm.) was given, and the longest once interposed between them, a similar effect upon the decidedness of the judgment was apparent.

It will not escape notice that in the case of observer R. a curious change of judgment manifests itself in Series IV A as compared with Series I and II. In the latter, with the standard horizontal, the illusion amounted to nearly 2 mm., whilst in Series IV A it amounted only to less than 1 mm. It might be supposed that the change was due to the influence of practice. As, however, it did not take place gradually, but evinced itself in the first day's observations of Series IV, this explanation can hardly be the right one.

---

[1] *Report of Cambridge Expedition to Torres Straits*, Vol. ii. pp. 115 and 127, and *Brit. Journ. Psych.* Vol. i. pp. 361 and 372.

Incidentally, our experiments throw an interesting light upon a matter over which a good deal of discussion has recently taken place. Prof. Stout contends that we owe to Stumpf an argument which definitely settles that there can be such a thing as *mere* sensation without cognitive function. "It is based," so Stout writes, "on the fact that within limits we can vary a stimulus without producing any perceptible difference in the object cognised. If this variation in the stimulus is accompanied by variation in the sense-experience, then we have a variation in the sense-experience which makes no difference to cognition. There is a difference in mere sensation, but not in perception. That, as a matter of fact, this is so may be demonstrated as follows. We may vary the physical conditions on which the pitch of a musical note depends, so as to produce a graduated scale of notes increasing or decreasing in pitch. Symbolise the series by P_1, P_2, P_3, P_4, P_5, ..., P_n. Now, if the variation of the physical conditions is sufficiently gradual, P_1 may be quite indistinguishable from P_2, and similarly P_2 may be quite indistinguishable from P_3, and P_3 from P_4. None the less, P_4 will be perceived as distinctly different from P_1. But this would be impossible unless the change in the physical conditions were accompanied by a change in the sensation, even when the change is imperceptible. If the pitch-sensation P_1 is regarded as identical with the pitch-sensation P_2, merely because the one note is indistinguishable from the other, and if in like manner P_2 is regarded as identical with P_3, and P_3 with P_4, and so on, then P_1 must be identical with P_n, and it would be impossible that any perceptible difference should ever arise. The same argument may be applied to a gradual increase in heat or weight or pressure or brightness[1]." We were working with excessively small increments of length, but the tables show that almost invariably a single increment, excessively small though it was, made a considerable difference to the recorded judgments. Adopting Prof. Stout's symbols for our increments of length, it would have been impossible to say that P_1 was ever "quite indistinguishable" from P_2, or that there ever was "a variation in the sense-experience which made no difference to cognition." As a matter of fact, judgments of identity or sameness were never really obtained;

---

[1] *Manual of Psychology*, pp. 120–1.

there was always more or less of wavering towards one side or the other. And a single increment produced an appreciable difference in respect to such wavering. The argument adopted by Stout presupposes that a cognition of identity is natural in such circumstances, but this is an assumption, and an assumption, moreover, for which it would be hard to find justification.

We revert now to what at the beginning of this article we indicated was the main object of our inquiry. The observations of which an account has been given seem to yield a definite and an unambiguous answer to the question from which we set out. They appear to point unmistakably to the conclusion that the illusion we have been studying is not due to the influence of movement factors. We have shown that the illusion is present in at least an equal measure when no such eye movements as would be necessary did estimation of length depend upon them could conceivably have taken place. We have shown, further, that, under these conditions of exclusion, judgments as to the relative lengths of horizontal and vertical lines are formed with greater definiteness and with much less hesitation than is the case under ordinary conditions. It is difficult to see how these facts can be reconciled with the hypothesis that ascribes the illusion to the influence of eye movements.

At the same time, we are quite aware of a possible objection that may be urged against our conclusion. It may be objected, namely, that although movements of the kind supposed were not actually carried out in the apprehension of the momentary exposed figure, yet for all that the genesis of the illusion may have been due originally to such movements, and that, in our experiments, we could not exclude the influence of previously acquired experience. Such, no doubt, is the objection Wundt would raise to the experiments in which we have been engaged. "If," he says, "a phenomenon is *only* perceived through means of the movement of the eye, then clearly the influence of movement upon that phenomenon has been demonstrated; we cannot, however, reverse the argument and conclude, as is sometimes done, that movement has no influence upon a phenomenon which continues to be present when the eye is at rest[1]." We have no desire to minimise any of the force which this argument may be deemed to possess. We fully admit that we have not excluded the possibility of appealing to movement factors so far as the problem of genesis is concerned. But the problem of genesis can only be satisfactorily dealt with on the basis of an analytic investigation of the developed consciousness. We have shown strong reasons for thinking that in developed experience the illusion is not dependent upon movement factors. If, notwithstanding, it be contended that such factors are essential for explaining its genesis, then the *onus probandi* rests with those by whom this contention is advanced.

---

[1] *Grundzüge*, 4te Aufl., Bd. ii. S. 139.

We desire to express our thanks to Mr E. Bullough of St John's College, Cambridge, to Mr L. J. Lamplugh, also of St John's College, and to Mr C. Scott of Cambridge, for their kind assistance as observers in these investigations.

THE INFLUENCE OF SMALL DOSES OF ALCOHOL ON THE CAPACITY FOR MUSCULAR WORK.

By W. H. R. RIVERS and H. N. WEBBER.

Experimental methods. Use of control substances. Doses varying from 5 to 20 c.c. of pure alcohol without effect on the capacity for muscular work when the factors of interest, sensory stimulation and suggestion are excluded.

It may seem that some apology is needed for publishing an account of the influence of a drug on the capacity for muscular work in a journal of psychology. The chief interest of our work, however, lies in the demonstration we give of the part played by purely psychical factors in the mode of measuring muscular work which is usually employed in the investigation of the action of drugs, and the chief importance of the work lies in its contribution to the study of the methods by which the influence of these psychical factors may be excluded.

Most of those who have tested the action of alcohol on the capacity for muscular work in man have obtained very definite and positive results with small doses, and we hope to show that these results have almost certainly been due to the neglect of certain factors which have influenced their work. We do not propose to give a full account of this previous work with small doses of alcohol[1] but are content to say here that some investigators (Lombard[2], Rossi[3], Frey[4], and in some persons, Joteyko[5]) have found the amount of work decidedly increased after doses varying from 5 to 20 c.c. of absolute alcohol; others (Destrée[1], and in some persons, Joteyko) have found a decided decrease with these doses, while others again (Scheffer[2] and Féré[3]) have found an initial increase followed by a fall below the normal. Others again, such as Schumburg[4] and Schnyder[5], obtained positive results, but found that these varied in nature according to certain conditions of the experiment.

---

[1] A full account of the history of the subject has been given by one of us in the Croonian Lectures given at the Royal College of Physicians in 1906 (to be published shortly).

[2] *Journ. Physiol.* 1892, Vol. XIII. p. 49.

[3] *Rivista sper. di Freniatria*, 1894, Vol. XX. p. 448.

[4] *Mitth. aus Kliniken u. med. Instituten d. Schweiz*, 1896, Reihe IV. Hft. I. and *Alkohol u. Muskelermüdung*, Leipzig u. Wien, 1903.

[5] *Travaux du Laboratoire de Physiologie* (Instituts Solvay), 1904, T. VI. p. 361.

All these investigators have compared the work done on days when alcohol was taken with that of other days when nothing was taken, and none of them have used on the normal days a control substance which would have prevented the subjects under experiment from knowing whether any given day was one on which the action of the alcohol was being tested or was one of the normal days.

That psychological factors play a part in the measurement of muscular work has been acknowledged by most investigators, but they have usually considered those coming under the heading of suggestion only and even the knowledge of this influence has not led them to adopt any means for its elimination. Féré[6] has drawn attention to the great influence of sensory stimulation on the capacity for muscular work, but even here it does not seem to have occurred to him to employ any adequate controls in his extensive researches on the action of drugs. No previous workers, so far as we know, have paid any attention to what we believe to be a far more potent source of error than either suggestion or sensory stimulation, viz., the interest and excitement produced by the act of taking the alcohol, and much of the work on the action of alcohol has been done by giving this substance, not in the pure state, but in one of the forms in which it is taken in everyday life—a procedure calculated to enhance the part taken by interest.

The special feature of our work is that it has been done with the aid of control substances so that we have disguised from ourselves whether, on any given occasion, we were taking alcohol or not, and have thus eliminated all possible effects of suggestion, sensory stimulation and interest.

Owing to the special flavour and pungency of alcohol it was not easy to discover adequate means of control. In this we owe very much to the kind help of Dr W. E. Dixon who in most cases prepared our mixtures for us, lettering them only, so that it was not till after an experiment was concluded that we learnt which of the bottles we had been given contained alcohol. The most successful control mixture was found to be one containing capsicum, cardamoms, chloroform and peppermint, these substances being also put into the mixtures containing the alcohol. Further in order to assist the control, we always took in any experiment two different doses of alcohol. If there were any effect, it should vary with the dose, while the fact that two different strengths of alcohol were being taken, helped to complete the disguise.

---

[1] *Journ. méd. de Bruxelles*, 1897, nos. 44 and 47.

[2] *Arch. f. exp. Pathol. u. Pharmakologie*, 1900, Bd. XLIV. S. 24.

[3] *Travail et Plaisir*, Paris, 1906, p. 301.

[4] *Arch. f. Anat. u. Physiol.*, Physiol. Abth. Suppt. Bd. 1899.

[5] *Arch. f. d. ges. Physiologie*, 1903, Bd. XCIII. S. 45.

[6] *Op. cit.*

In most of our experiments the disguise was completely successful. Occasionally we might suspect the presence of alcohol, either from taste or from sensations localised in the epigastrium, but these were in general vague and often incorrect; only in one experiment did the control fail entirely. With the doses with which we are concerned in this article we did not become aware that we had taken alcohol by means of any after-effects as has often been the case with larger doses. Any differences in taste were so slight that we certainly excluded the influence of sensory stimulation, and even when the presence of alcohol was suspected there was so much uncertainty that the influence of suggestion may also be confidently excluded. There remains the effect of interest. In one case there is little doubt that the interest aroused by (correct) suspicions as to the nature of the dose increased the amount of work, but in all other cases the disguise was either so complete or the suspicions awakened were so indefinite as to make it very unlikely that there were differences in interest sufficient to affect the amount of work.

We used the modification of Mosso's ergograph devised by Kraepelin, a very perfect instrument, in which various sources of error which vitiate much work on the action of alcohol are entirely avoided, the most essential features being the absolute guarantee that the movements throughout are limited to one joint and that the point of application to the finger of the cord carrying the weight is always at the same distance from this joint.

Another feature of our method, which has been used by Frey but not by other workers on alcohol, was to test the action of the drug on sets of ergograms separated by intervals of rest. In our earlier work we recorded three sets, each of six ergograms, at half-hour intervals, while in our later experiments a larger number of sets were recorded with longer periods of rest between the later sets. In all cases the interval between the ergograms of a set was two minutes and the rhythm of each ergogram was one of two seconds, the finger being flexed with one beat and relaxed with the next beat of a metronome beating seconds. In some experiments the dose, whether of alcohol or 'control,' was taken shortly before the beginning of the second set of ergograms; in other experiments it was taken during the interval between the first and second ergograms of the first set. Thus the first ergogram of every day was taken under similar conditions, and in order to show the action of the drug we always express the work of every ergogram in percentages of the work of the first ergogram. The weight used in all the experiments recorded in this paper was 4·5 kg.

Each experiment lasted for a number of days on some of which one dose of alcohol was taken, on others a different dose and on the rest, either the control mixture or nothing at all. Every effort was made to keep the different days as alike as possible; the work was begun at the same time every day, at the same interval after a meal of the same kind and amount. This latter point is very important, for Schnyder has found that there is a difference in the action of alcohol according as it is taken during or soon after a meal or at a considerable interval, and in our experiments the work was always begun half an hour after a light meal. The other conditions of life on the different days of the experiment were kept as equable as possible; the same amount of other work was done, the same amount of exercise was taken at the same times, and such disturbing factors as social engagements were eschewed. As far as possible the same amount of sleep was taken each night.

Further, during the whole course of the experiments lasting over many months all articles of diet containing caffeine or alcohol were avoided, and by one of us (R.) had been avoided for more than a year before the experiments began. The other of us (W.) had always been a total abstainer from alcohol.

The experiments were preceded by some initial training which is very necessary in the use of the ergograph, but we did not wait to begin our experiments till we were completely trained, so that our work has been done in different conditions of training.

In the case of one of us (R.) the work with the ergograph was much hampered by muscular pain, and partly for this reason, partly because his time could not be wholly given to the work, the experiments of the other (W.) are more extensive and more satisfactory and will be considered first.

The experiments of the subject W.

Experiment i. In this experiment three sets of ergograms were recorded with half-hour intervals between the sets. During the intervals the subject adjusted the apparatus and took readings while the other of us was carrying out an experiment, so that he was occupied in mental and muscular work of a gentle kind. The doses of alcohol were 5 and 10 c.c., and both these and the control mixture were taken in a tumbler of water ten minutes before the beginning of the second set. The experiment was conducted in March, 1906, on the mornings of twelve days half an hour after a light breakfast. On six days the right hand was used and on the other six the left hand. There was no difference between the results with the two hands, and the figures are grouped together in the following table and curves.

TABLE I. *Experiment W.* i. *Work. Average day. Both hands. Weight* 4·5 *kg. Dose* 10 *minutes before second set.*

	First set			Second set			Third set		
Ergogram	Control.	5 c.c. Alcohol	10 c.c. Alcohol	Control	5 c.c. Alcohol	10 c.c. Alcohol	Control	5 c.c. Alcohol	10 c.c. Alcohol
1	5·1 (100)	6·2 (100)	6·2 (100)	3·4 (66)	3·9 (64)	4·0 (66)	3·3 (63)	2·7 (43)	3·8 (62)
2	1·9 (35)	2·0 (34)	2·4 (40)	1·2 (22)	1·4 (24)	1·5 (24)	1·2 (21)	1·3 (20)	1·6 (26)
3	1·5 (27)	1·5 (26)	1·6 (26)	0·9 (15)	0·9 (15)	1·1 (18)	1·0 (18)	1·0 (14)	1·4 (22)
4	1·3 (23)	1·6 (25)	1·5 (25)	0·8 (18)	0·7 (13)	1·3 (21)	0·9 (16)	0·8 (12)	1·0 (16)
5	1·2 (21)	1·2 (20)	1·4 (22)	0·9 (16)	0·5 (8)	0·9 (14)	0·8 (18)	0·7 (10)	1·1 (18)
6	1·2 (23)	1·0 (16)	1·3 (20)	0·8 (14)	0·5 (9)	0·8 (13)	0·9 (14)	0·7 (11)	0·9 (14)
Total	12·2 (100)	13·5 (100)	14·4 (100)	8·0 (62)	7·9 (59)	9·6 (67)	8·1 (62)	7·2 (49)	9·8 (66)

In Table I. we give in kilogrammetres the work of the average ergograms, each figure showing the average of the ergograms of four days. As the dose, whether of alcohol or control, was not taken till ten minutes before the beginning of the second set, the ergograms of the first set were taken under the same conditions on the three groups of days and they should, therefore, correspond. It will be seen that in absolute amount the figures on the control days are rather smaller than on the two alcohol days, owing to some difference of disposition on the different days. The course of the ergograms of each set however, as shown by the figures in brackets, which give the amounts as percentages of the first ergogram, run very closely together. This is also shown in the lowermost curves of Fig. 1 where the course of the ergograms is shown graphically, and the correspondence of the three curves for the work of the first set is as close as could be desired. The total amounts for the work of the second set, given in the bottom line of the table, show that rather

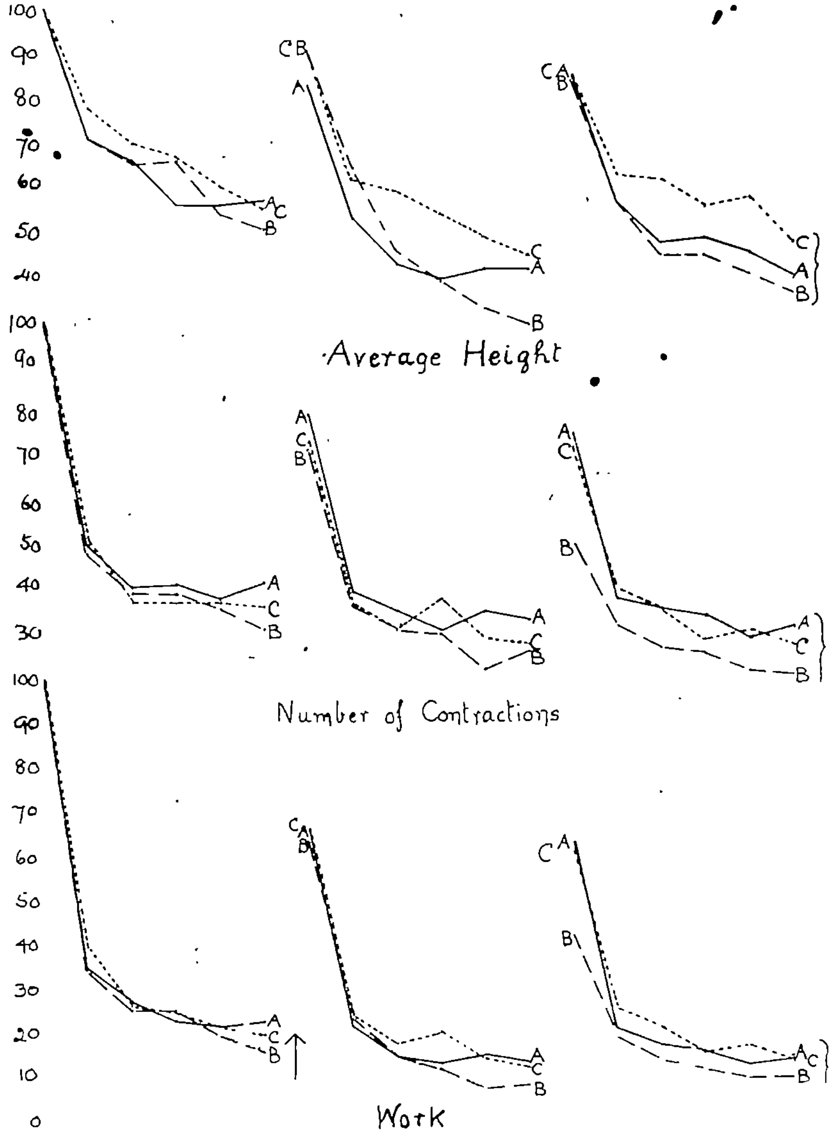


FIG. 1. Experiment *W.* i. *A*, Control mixture. *B*, Alcohol 5 c.c. *C*, Alcohol 10 c.c. Arrow shows when the dose was administered.

less was done on the days on which only 5 c.c. were taken than on the other two groups of days, but for this set also Fig. 1 shows that there is very little difference in the course of the three curves, *i.e.* there is no indication of any effect produced from ten to twenty-five minutes after taking the alcohol. In the case of the third set there is a very close correspondence between the curves for the control days and those on which the larger dose of alcohol was taken, but the curve for the days on which only 5 c.c. were taken falls below the others, especially for the first ergogram, and the total amount of work for this set is distinctly less than on the other two groups of days.

It is very unlikely that a dose of 5 c.c. would produce a diminution in the amount of work while double the dose has no effect, and we can have little hesitation in ascribing the nature of the *B* curve to some accidental factor. The other curves of Fig. 1 give the course of the numbers of contractions and of the average heights of the contractions respectively, the latter being obtained by dividing the total height through which the weight was lifted by the number of contractions. It will be seen that there is no definite indication of an alcohol effect on the number of the contractions except in the third set, when it becomes clear that the course of the *B* curve for the work was determined chiefly by this feature of the ergogram.

The curves representing the height of the contractions, on the other hand, though far less regular than those for either the amount of work or the number of contractions, show an increase on the days on which 10 c.c. of alcohol was taken, but it will be noted in Fig. 1 that the curve representing the height was showing a distinct tendency to rise on these days in the first set, *i.e.* before any dose had been taken, and it is doubtful whether any significance can be attached to the rise of this curve in the remaining sets[1].

Experiment ii. This lasted for six days, from April 8—13. Three sets of six ergograms were recorded at half-hour intervals and the dose, whether of alcohol or control, was taken between the first and second ergograms of the first set, the doses of alcohol being 5 and 10 c.c. as in the first experiment. The work was done in the afternoons instead of in the mornings, but at the same interval after a light lunch corresponding in kind and amount to the breakfast of the first experiment. The intervals were occupied with the task of multiplication, using for this purpose the *Rechenhefte* of Kraepelin[2].

---

[1] The inferiority of the *A* and *B* curves as regards the height of the contractions is almost entirely due to the two first days of the experiments when the contractions became very small towards the end of the day.

[2] The special methods and results of this work will be recorded elsewhere.

In this and all succeeding experiments the subject took his own readings, the two minutes' interval between successive ergograms affording ample time for this and for any necessary adjustments of the ergograph. That the subject should take his own readings has some disadvantages, but it was not possible for another person to be present to record them in all cases, and unless this could be done in all cases, it was thought best that it should be done in none. The whole business of reading the scale and making any necessary adjustments so soon settles down to a steady routine that the subject is not led to think about the relations of the readings he is recording, and we are not at all sure that the method of taking one's own readings is not less disturbing than if the subject has two minutes of complete leisure between each pair of ergograms for reflexion on the nature of his performance. Further, there is the great advantage that the presence of another person in the experimental room, with all the possible sources of disturbance involved, can be dispensed with.

TABLE II. *Experiment W. ii. Work. Average day. Left hand. Weight 4·5 kg. Dose one minute after first ergogram.*

	First set			Second set			Third set		
Ergogram	Control	5 c.c. Alcohol	10 c.c. Alcohol	Control	5 c.c. Alcohol	10 c.c. Alcohol	Control	5 c.c. Alcohol	10 c.c. Alcohol
1	6·8 (100)	6·7 (100)	6·9 (100)	4·6 (69)	6·8 (102)	6·8 (99)	5·3 (79)	5·9 (88)	4·8 (70)
2	2·8 (42)	3·0 (45)	4·0 (58)	3·1 (47)	3·1 (46)	3·2 (46)	3·0 (45)	2·4 (37)	2·4 (36)
3	1·9 (28)	2·6 (40)	3·2 (46)	2·8 (41)	3·1 (47)	2·3 (34)	2·4 (35)	2·6 (39)	2·6 (38)
4	2·1 (31)	2·7 (41)	2·5 (36)	2·2 (32)	2·6 (39)	2·3 (34)	2·3 (34)	2·5 (38)	2·2 (32)
5	1·7 (24)	2·3 (35)	2·8 (40)	2·4 (36)	2·3 (34)	2·1 (31)	1·8 (27)	2·4 (35)	1·8 (26)
6	2·0 (30)	2·7 (40)	3·1 (45)	2·3 (35)	3·1 (46)	2·1 (30)	1·9 (28)	2·5 (38)	1·9 (28)
Total	17·3	20·0	22.5	17·4	21·0	18·8	16·7	18·3	15·7

The special interest of this experiment lies in the failure of the control. The larger dose of 10 c.c. was definitely recognised on each occasion and the presence of alcohol was suspected in the case of the smaller dose. The figures given in Table II. and those represented graphically in Fig. 2 show a definite increase on the alcohol days, greater on those when the larger dose was taken, and this increase was apparent immediately after the dose was taken. It continued to the end of the first set and was also distinctly present in the first ergogram of the second set, this curve falling slightly below that of the normal days in the remaining ergograms. The curves for number and height of the contractions show that the increase in the amount of work on the alcohol days was chiefly due to an effect on the former, though there was also a slight increase in the height in the first set on the two alcohol days, the curve for the smaller dose, however, here rising above that for the larger.

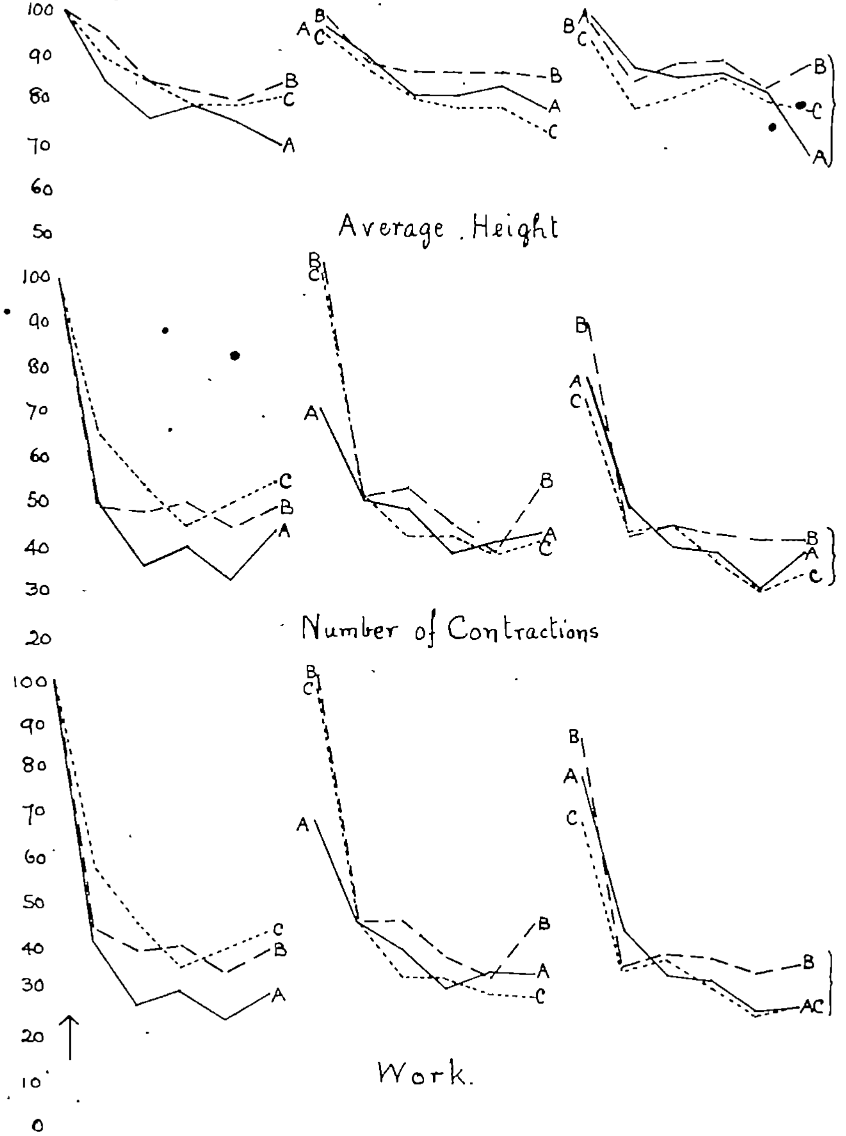


FIG. 2. Experiment *W.* ii. *A*, Control mixture. *B*, Alcohol 5 c.c. *C*, Alcohol 10 c.c.

The action of alcohol in this experiment was of the same kind as that which has been described by most previous workers, viz., an immediate increase followed by a reaction shown in a fall below the normal, and it is interesting that this kind of action should have shown itself in an experiment in which there was a failure of the control, so that the feature of interest would be awakened.

Experiment iii. The control having failed in the second experiment, it became necessary to modify the mixtures used for this purpose. The mixtures had so far contained capsicum, cardamoms and chloroform only, to which Dr Dixon now added some peppermint. The new control was completely successful, the disguise being so perfect that in this and succeeding experiments the subject was wholly unaware, so far as taste was concerned, whether he had taken alcohol or the control mixture.

The experiment was begun on May 7 with the idea of testing the influence of doses of 10 and 20 c.c. of absolute alcohol by exactly the same methods as in the second experiment, except that the intervals on half the days were to be occupied with multiplication while on the other half the place of the multiplication was to be taken by type-writing. At the end of five days the experiment was unavoidably interrupted by which time only one day's work had been done with each dose. The smaller dose of 10 c.c. had given a completely negative result while with the larger dose of 20 c.c. there had been an increase only noticeable in the first ergograms of the second and third sets. As there did not seem evidence of any definite effect we decided not to repeat this experiment but to proceed directly to larger doses, especially as the dose of 20 c.c. was to be tried in the next experiment.

Experiment iv. This lasted for twenty days, on six of which no dose of any kind was taken, on five a control mixture was taken, while on the other days the doses of alcohol were either 20 or 40 c.c. of absolute alcohol. Each dose was taken between the first and second ergograms of the first set.

On each day four sets were recorded, each of six ergograms. The interval between the first and second and between the second and third sets was half an hour, but between the third and fourth sets it was increased to one hour. The first and second intervals were occupied in either multiplication or type-writing, the amount done in each five minutes being recorded with the help of a five minutes' clock. This work was done for twenty-five minutes, leaving time at the beginning and end for the necessary adjustments of the ergograph. The third interval of an hour was passed in gentle walking in the open air and in light reading, while a little food was also taken.

Another modification introduced into this experiment was that the work with the ergograph was done in two ways. On some days the finger was allowed to contract to the full extent in the ordinary way; on the other days an obstacle was placed in the course of the contracting finger so that the contractions were only allowed to reach two-thirds of their normal extent. We shall speak of these methods as those of complete and incomplete contractions respectively[1].

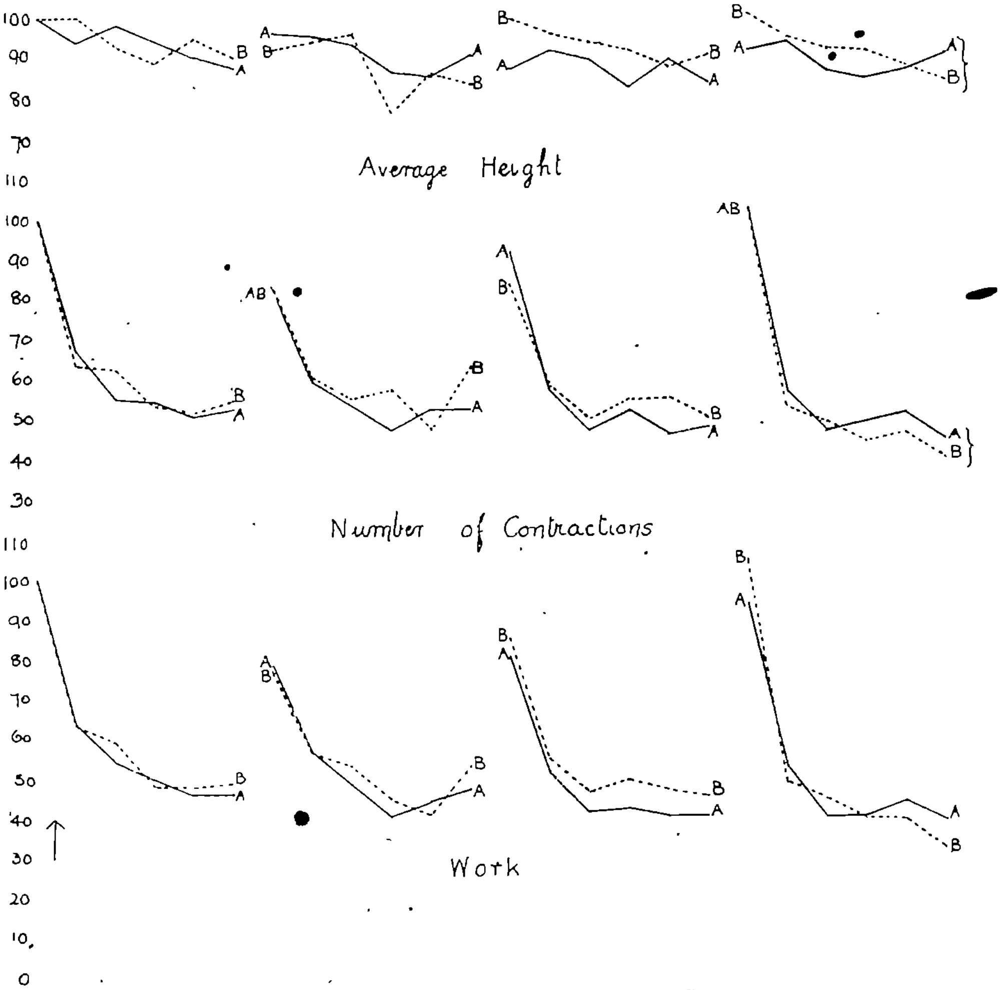


FIG. 8. Experiment *W*. iv. Complete contractions. *A*, Control mixture. *B*, Alcohol 20 c.c.

---

[1] The consideration of the motive for this procedure and the different results of the two procedures must be deferred for another occasion.

As in this paper we are dealing only with small doses of alcohol, we give in the following tables and curves the results for the smaller dose of 20 c.c. and leave the results with the larger dose of 40 c.c. for a later communication. The figures for the smaller dose with the two kinds of contraction are given separately.

TABLE III. *Experiment W.* iv. *Work. Complete contractions. Left hand. Weight* 4·5 *kg. Dose taken one minute after first ergogram.*

	First set		Second set		Third set		Fourth set	
Ergogram	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol
1	6·6 (100)	5·4 (100)	5·1 (78)	4·2 (77)	5·2 (80)	4·6 (84)	6·1 (93)	5·7 (104)
2	4·2 (64)	3·4 (63)	3·7 (57)	3·0 (56)	3·4 (51)	2·9 (54)	3·4 (52)	2·6 (48)
3	3·6 (54)	3·2 (59)	3·2 (48)	2·9 (53)	2·7 (41)	2·5 (46)	2·6 (39)	2·4 (44)
4	3·3 (50)	2·6 (48)	2·6 (40)	2·4 (44)	2·7 (42)	2·6 (49)	2·6 (39)	2·1 (39)
5	3·0 (46)	2·6 (48)	2·9 (44)	2·2 (40)	2·6 (40)	2·5 (46)	2·8 (43)	2·1 (39)
6	3·0 (46)	2·6 (49)	3·1 (47)	2·8 (52)	2·6 (40)	2·4 (45)	2·5 (38)	1·7 (31)
Total	23·7	19·8	20·6	17·5	19·2	17·5	20·0	16·6

The figures for the amount of work with the complete contractions given in Table III. and represented graphically in Fig. 3 show not the slightest trace of an immediate effect, and the two curves for the first set run together as well as if no drug had been given. In the second set there is also no definite indication of an effect, which only appears in the third set begun an hour and a half after the alcohol had been taken. The fact that the alcohol curve in this set is consistently above that for the normal days suggests that we may possibly have to do here with a real drug effect, though of only slight amount, which has wholly disappeared an hour later. The other curves show that this increase is due to an effect on both number and height of the contractions, perhaps chiefly on the former.

The results with the incomplete contractions shown in Table IV. and Fig. 4 seem to show an immediate depressing effect of the alcohol, but not of any great amount. In the second set this is replaced by an increase on the alcohol days, which becomes still more definite in the third set to disappear in the last. This correspondence with the complete contractions in showing a definite increase in the third set is almost certainly significant, and seems to point to a stimulating action of the alcohol asserting itself most definitely about an hour and a half after its administration.

This occurrence of a late increase with the incomplete contractions makes it probable that the fall of the curve in the first set is due to some accidental circumstance; certainly there was no increase of the kind which has usually been recorded.

TABLE IV. *Experiment W. iv. Work. Incomplete contractions. Left hand. Weight 4·5 kg. Dose taken one minute after first ergogram.*

	First set		Second set		Third set		Fourth set	
Ergogram	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol
1	7·1 (100)	5·9 (100)	4·6 (61)	3·8 (62)	3·9 (53)	4·3 (73)	5·2 (69)	4·4 (72)
2	4·0 (58)	2·8 (48)	2·8 (38)	2·7 (45)	2·9 (41)	2·6 (43)	4·4 (54)	2·7 (45)
3	3·6 (50)	2·3 (40)	2·7 (36)	2·4 (40)	2·0 (28)	2·2 (37)	1·8 (26)	2·1 (35)
4	3·3 (44)	2·3 (38)	2·8 (38)	2·0 (35)	1·8 (25)	2·3 (37)	2·0 (27)	2·0 (33)
5	2·8 (37)	2·2 (37)	1·8 (25)	2·0 (33)	1·9 (26)	2·2 (37)	2·2 (29)	2·1 (35)
6	2·9 (39)	2·1 (35)	2·3 (31)	2·7 (45)	2·9 (39)	1·9 (31)	2·1 (28)	1·8 (30)
Total	23·7	17·6	17·0	15·6	15·4	15·5	17·7	15·1

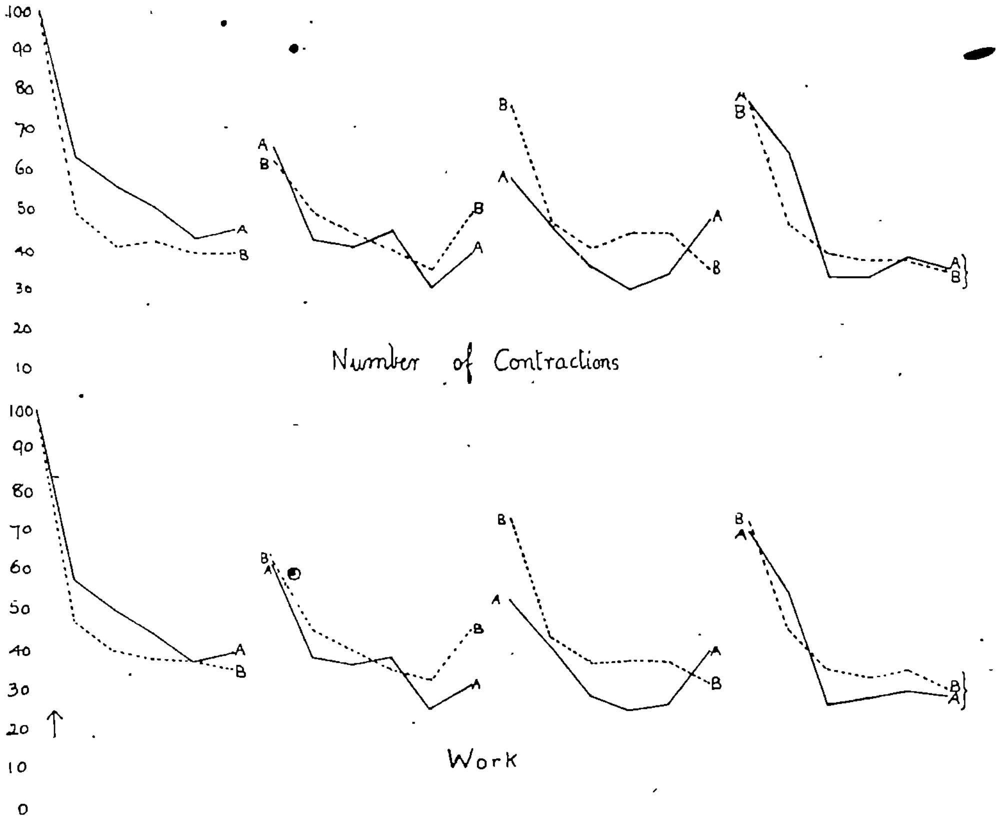


FIG. 4. Experiment *W.* iv. Incomplete contractions. *A*, Control mixture. *B*, Alcohol 20 c.c.

This fourth experiment thus confirms the first and third in showing no trace of the immediate stimulating action when there is an adequate control and any increase which is found is considerably delayed, being most pronounced about an hour and a half after the alcohol has been taken.

The only definite increase thus occurs at a time when the effect described by most previous workers has disappeared or at a period beyond that to which they have followed the effects of the drug. The reality and nature of this late effect must be left for future work and we wish to dwell especially on the absence of the immediate stimulating effect.

A word must be said here on the validity of the procedure of expressing the results in their relation to the first ergogram. In the first experiment, and in the experiments with caffeine which we have recorded elsewhere[1], the dose of drug or control was not taken till after the first set had been completed so that we are able to express the course of the work in terms of the whole of the first set as well as in terms of the first ergogram, and the correspondence of the two ways of expressing the results in those cases can leave little doubt about the genuineness of the results.

In the later alcohol experiments of W. the dose was taken immediately after the first ergogram so that in this case we have only one ergogram to act as a standard. As a general rule, however, the course of the ergograms of the first set on the normal days is so constant and the later ergograms stand in so definite a relation to the first that we have little doubt of the validity of our procedure. Still in such a case there must always be the possibility that the first ergogram may be exceptional in some way, and in later experiments with other subjects we have not given the drug or control till a whole set had been recorded to act as a standard.

The experiments of the subject R.

The experiments of this subject were much less satisfactory than those of W. owing to the muscular pain produced when sets of six or even of three or four ergograms were recorded. His first experiment in March, 1906, was planned to correspond with the first experiment of W., doses of 5 and 10 c.c. of alcohol being taken ten minutes before the beginning of the second set. This experiment could only be continued for five days, the results of which, though irregular, gave no indication of an alcohol effect.

---

[1] *Journ. Physiol.* 1907, Vol. XXXVI. p. 33.

Experiment ii. It was not possible to do any further satisfactory work till August of the same year, when after prolonged training an experiment with incomplete contractions lasting for six days was carried out with doses of 10 and 20 c.c., taken one minute before beginning the second set. Five sets were recorded, each of four instead of six ergograms as usual. As in our other experiments the two first intervals were half an hour and those between the later sets an hour, and were passed in some light occupation, or in the open air, the corresponding intervals on different days being always occupied in the same manner.

The results are given in Table V. The figures for the individual ergograms are extremely irregular, for the amount of practice at this stage was still insufficient and the contractions often produced pain. The figures of the bottom line of the table, which give the total work of each set, show little difference in the work of the first sets on the three groups of days, before any dose had been taken, and there is no difference in the figures for the second set begun one minute after administration of the dose, *i.e.* there is no trace of any immediate effect. The figures for the third set, on the other hand, seem to show an alcohol effect, the increase being greater with the larger than with the smaller dose. The fourth set shows no trace of an alcohol effect, while the individual ergograms of the last set are so extremely irregular that little importance can be attached to the differences in the totals, especially as there is little difference between the amounts for the control days and those on which the larger dose was taken. The only indication of an effect is, therefore, in the third set, begun three-quarters of an hour after the alcohol was taken, and the result of the succeeding experiment makes it doubtful whether the difference was due to anything more than the irregularity caused by the imperfect condition of training.

Experiment iii. From Aug. 27 to Sept. 1, 1906, a six days' experiment was carried out in which five sets, each of six ergograms, were recorded with the right hand and with complete contractions. The doses of alcohol were 20 and 40 c.c. and they or the control were taken one minute before beginning the second set of ergograms. Even with the larger number of ergograms in each set, and though the contractions were complete, there was less pain than in the second experiment, and the form of the curves showing the course of the individual ergograms (see Fig. 5) is far more regular than in that experiment, though still far from attaining the regularity of the fully trained condition. As in the case of W. the results with the larger dose will be left for another

TABLE V. *Experiment R.* ii. *Work. Incomplete contractions. Right hand. Weight* 4·5 *kg. Dose taken one minute before second set.*

	First set			Second set			Third set			Fourth set			Fifth set		
Ergogram	Control	10 c.c. Alcohol	20 c.c. Alcohol	Control	10 c.c. Alcohol	20 c.c. Alcohol	Control	10 c.c. Alcohol	20 c.c. Alcohol	Control	10 c.c. Alcohol	20 c.c. Alcohol	Control	10 c.c. Alcohol	20 c.c. Alcohol
1	2·0 (100)	1·9 (100)	2·2 (100)	2·6 (135)	2·3 (118)	2·4 (130)	1·7 (85)	2·0 (105)	2·4 (138)	2·1 (108)	2·4 (122)	2·1 (100)	0·8 (46)	1·8 (94)	1·0 (59)
2	2·5 (142)	2·6 (134)	2·7 (148)	2·1 (113)	2·1 (109)	1·7 (94)	1·7 (80)	2·1 (107)	2·7 (146)	1·9 (98)	2·5 (127)	2·4 (135)	1·5 (79)	2·3 (118)	2·2 (133)
3	2·6 (145)	2·1 (110)	2·7 (150)	2·1 (111)	1·9 (96)	2·4 (137)	2·0 (109)	2·1 (107)	2·0 (116)	2·4 (124)	1·8 (98)	2·6 (144)	2·1 (109)	2·1 (110)	1·3 (61)
4	2·0 (110)	2·7 (139)	2·2 (113)	1·7 (78)	2·4 (122)	2·0 (108)	1·4 (70)	1·9 (96)	1·8 (98)	2·2 (114)	1·8 (98)	1·9 (106)	2·2 (130)	2·1 (107)	2·0 (106)
Total	9·1 (100)	9·3 (100)	9·8 (100)	8·5 (91)	8·7 (91)	8·5 (89)	6·8 (73)	8·1 (86)	8·9 (94)	8·6 (92)	8·5 (90)	9·0 (98)	6·6 (72)	8·3 (89)	6·5 (68)

TABLE VI. *Experiment R.* iii. *Work. Complete contractions. Right hand. Weight* 4·5 *kg. Dose taken one minute before second set.*

	First set		Second set		Third set		Fourth set		Fifth set	
Ergogram	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol	Control	20 c.c. Alcohol
1	4·1 (100)	3·7 (100)	3·7 (91)	4·1 (109)	3·7 (91)	4·1 (112)	4·0 (100)	3·7 (101)	4·5 (117)	3·8 (102)
2	5·0 (126)	4·3 (116)	4·2 (105)	4·5 (124)	4·1 (98)	3·4 (92)	3·5 (88)	3·5 (97)	4·0 (102)	3·4 (92)
3	3·7 (91)	4·0 (110)	3·5 (88)	3·5 (93)	3·4 (84)	3·3 (91)	3·5 (93)	3·2 (88)	3·2 (81)	3·2 (87)
4	4·1 (104)	3·5 (97)	3·3 (85)	3·3 (90)	3·2 (81)	3·2 (88)	2·9 (75)	2·8 (78)	2·9 (74)	2·7 (74)
5	3·2 (82)	3·2 (87)	3·7 (96)	3·2 (88)	3·3 (83)	3·0 (83)	3·1 (78)	2·9 (80)	2·9 (74)	2·5 (70)
6	3·7 (91)	3·2 (88)	4·8 (123)	3·3 (90)	3·1 (79)	2·6 (72)	2·9 (73)	2·5 (69)	2·8 (72)	2·6 (70)
Total	23·8 (100)	21·9 (100)	23·2 (98)	21·9 (100)	20·8 (87)	19·8 (90	19·9 (84)	18·6 (86)	20·3 (86)	18·2 (83)

communication and the full results for the control days and those on which 20 c.c. were taken are given in Table VI.

The total amounts for the whole sets given in the bottom line of this table show no trace of an alcohol effect. The results for the individual ergograms, expressed graphically in Fig. 5, show what may be an alcohol effect in the second set begun almost directly after taking the dose, but the very abnormal form of the curve for the control days[1] shows that much weight cannot be laid on the record of this set. The first part of the control curve of the third set is also abnormal, probably owing to the existence of some pain, and it is only the first ergogram of

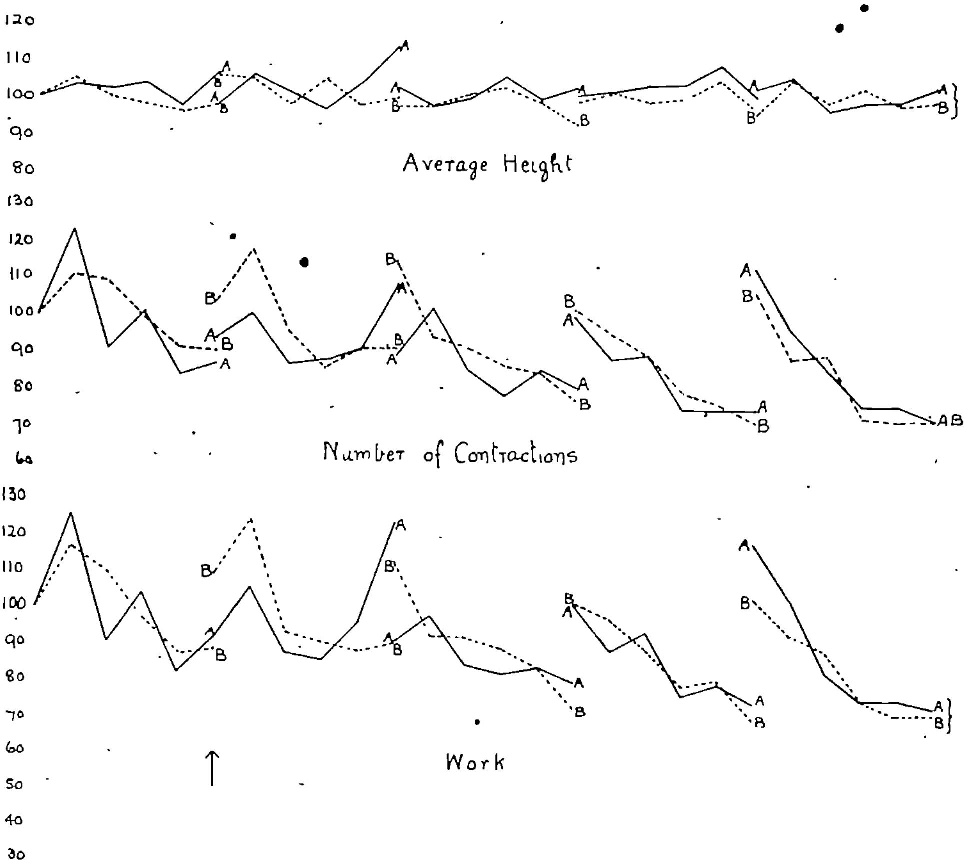


FIG. 5. Experiment *R.* iii. *A*, Control mixture. *B*, Alcohol 20 c.c.

---

[1] The great rise at the end of this set was chiefly due to a disturbance which occurred on one of the control days. The drum did not act properly and the mental excitement thus produced was followed, as usual, by a large increase in the amount of work.

this set in which there is any decided difference between the alcohol and control days, while in the fourth set the two curves run very closely together.

This experiment, then, differs from the preceding in giving some indication of an immediate effect, but, on the other hand, there is no trace of the increase after an hour and a half which occurred in the case of W. and the nature of the early increase gives one no confidence in its dependence on the action of the alcohol.

As in this experiment the dose was not taken till the first set of ergograms was concluded, it is possible to use the average ergogram of this set as a standard by means of which to express the course of the individual ergograms. We have therefore constructed curves resembling those of Fig. 5 except that each ergogram is expressed, not as a percentage of the first ergogram, but as a percentage of the average of the whole of the first set, *i.e.* an average obtained by dividing the total work of this set by six. The curves so constructed, however, were found to agree very closely in their general features with those already given.

Conclusions.

The chief result of our experiments is that we have failed to confirm the conclusion of nearly all previous workers that small doses of alcohol, from 5 to 20 c.c., have an immediate stimulating or depressing action on the capacity for muscular work as tested by the ergograph. Our experiments have differed from all previously recorded in the use of control substances by means of which we have been successful in rendering the days on which alcohol was taken indistinguishable from the normal days, and in the only case in which we have found a definite immediate effect there had been failure of our control. The natural conclusion is that the results of previous workers have been due, not to the physiological effects of the alcohol, but to the presence of certain factors, and especially interest and sensory stimulation, the influence of which we have been able to exclude.

Before, however, accepting this conclusion it is necessary to inquire whether there have been any other differences between our experiments and those of previous workers which might explain the divergent results.

One such difference is a consequence of our use of a control. The mixtures in which we took the alcohol for purposes of disguise were decidedly unpleasant and the possibility arises that this may have neutralised an effect which the alcohol might otherwise have produced. The control mixture containing no alcohol, however, was equally unpleasant, and as the unpleasantness was the same on both alcohol and control days, any alcohol effect which existed ought to have become evident. Further, in later experiments to be recorded on another occasion a different control has been used decidedly pleasant to the taste, and in these experiments also there has been no evidence of an alcohol effect.

Another possibility must be borne in mind. One of the most definite results of previous work on the action of alcohol has been to show the presence of great individual differences in the susceptibility to the action of the substance, and though it is possible that these variations have been largely due to differences in the part played by the psychical factors which it has been our business to exclude, there can be little doubt of the existence of individual differences in the true physiological action of the drug. It is possible that the two subjects whose work has been recorded in this paper are examples of insusceptibility to the action of alcohol. One of us knows, however, that he is unusually susceptible to the general effects of alcohol, and observations on the general condition of the other made during these and other experiments[1] show that he is certainly not an example of any special insusceptibility to the action of the substance.

A further piece of evidence that our negative results have been due to the use of an adequate control is derived from a short experiment on the subject W. with doses of whiskey containing 10 and 20 c.c. of absolute alcohol. These observations were made by giving the whiskey without any attempt at disguise, and it was found that both doses produced a very decided increase in the amount of work as compared with a normal day. The most noteworthy fact is that this increase was immediate exactly as in most previously recorded work. It is of course open to anyone to say that the increase in this case was due to some component of the whiskey other than ethylic alcohol, but the immediacy of its action can leave little doubt that we had to do mainly with the action of interest and sensory stimulation. If so, we have the clearest evidence that the cause of the negative results with pure alcohol in the case of the subject W. was not individual insusceptibility, but the means which were taken to exclude the psychical factors which have been allowed to influence all previous results.

---

[1] *Journ. Physiol.* 1907, Vol. XXXVI. p. 83.

There can, we think, be little doubt that the absence or slightness of the effects with small doses of alcohol which we have found are due to our use of control substances, and that the results of most of those who have found the capacity for muscular work increased under the influence of such doses as 5 and 10 c.c. have been due, not to the physiological action of the substance, but to the interest aroused by taking the alcohol and the sensory stimulation involved in swallowing it, while those who have found a diminution in the amount of work must be open to the charge of having been influenced by suggestion.

Our experiments show definitely that the factors of interest, sensory stimulation and suggestion may play a great part in the process of testing the action of a drug with the ergograph, and we hope to have shown that no future work on the action of drugs can be regarded as decisive if these factors have not been excluded by the use of an adequate control.

THE WHEATSTONE-HIPP CHRONOSCOPE.

A SECOND NOTE.

BY BEATRICE EDGELL AND W. LEGGE SYMES.

[*From the Physiological Laboratory of the University of London.*]

Observations on a fourth instrument. 'Octave error.' Constancy. Disadvantages of indirect control. Direct control by lever on armature of chronoscope.

SINCE publication of our paper[1] on the chronoscope, we have had the opportunity of testing a fourth instrument, fresh from the works of Messrs Peyer and Favarger.

On arrival, its escapement was found to be so well adjusted as to give no more than one false note in upwards of one hundred readings. For this reason, and because, when tested by the armature lever as described on pages 63, 73 and 82, it showed a clockwork error of less than ·2 °/₀, no attempt was made at this stage to readjust the vibrating spring.

A small breakage having necessitated its return to the makers, the foregoing tests were repeated on its rearrival. The escapement was now found to give about ten false notes per hundred readings. The vibrating spring, therefore, was readjusted on the lines laid down on p. 74, and has since then yielded more than a thousand readings without audible fault, and with a clockwork error distinctly less than that shown by the earlier measurement.

In the matter of constancy, it was found, as with the three other instruments, that the chronoscope behaves more uniformly when its hands run during flow of current. Thus, in one hundred consecutive readings of hammer time, the M. V. of the consecutive groups of ten, ranged from ·20σ to ·80σ when the hands ran during current flow; and in a similar series from ·54σ to 1·08σ when the hands ran during interruption of current.

---

[1] This volume, pp. 58–88.

Further use of the hammer in conjunction with the chronograph, for testing the accuracy of the chronoscope adjustments, has led us to modify our opinion as to its convenience. Making all allowance for fair wear and tear (*e.g.* failure of springs), the hammer has not proved durable. The arm bearing the counterpoise has snapped off, the screw thread[1] in the counterpoise has completely stripped, and the keys have, through bad wear[2], ceased to establish permanent closure at first contact. Moreover, tests of the chronograph (Pfeil) by the drum keys (p. 63), have shown us that an error of 3 or 4σ may be made in the estimate of an interval recorded by it, if the tension of its spring is not favourable. And although it can be so adjusted (p. 72), such adjustment requires periodical verification.

Since, then, appeal has ultimately to be made to these drum keys to insure accurate interpretation of the hammer-Pfeil combination; and since also these keys in conjunction with a lever on the armature of the chronoscope determine the error of the chronoscope readings directly,—employment of the intermediate controlling apparatus (hammer and Pfeil) is gratuitous.

The *modus operandi* of the drum method is figured and described on p. 63 of this volume and tracings are given in the figures on the two following pages. It is only necessary to add that the central horizontal line shown in these tracings, which was used to measure the latencies of the chronoscope, is not required for the purpose of measuring its error. This error is given by the difference between the interval shown on the time tracing from *M* to *B*, and the interval as recorded by the dials. The position of *M* and *B* is found, after each individual record, by turning the drum round by hand so slowly that at 'make' and 'break' it is practically stationary. This yields almost vertical lines between the upper and lower abscissae, from the beginnings of which parallels are drawn, in the usual way, to cut the tracing of the tuning-fork. The following is an example of the results given by the method.

---

[1] The counterpoise was found to consist of lead with a thin covering of brass.

[2] This would have been diminished, if not wholly prevented, by use of platinum-iridium alloy in place of the pure platinum actually employed.

TABLE I.

Graphic value of interval	Chronoscopic reading	Error of chronoscopic reading with a given adjustment
150 σ	158 σ	+ 8 σ
153	159	+ 6
154	161	+ 7
154	161	+ 7
154	162	+ 8
155	162	+ 7
155	161	+ 6
150	156	+ 6

The average error of the chronoscope with the adjustment in question was therefore (to the nearest whole number) 7σ, and the mean variation from the actual average rather over ·6σ. The varying duration of the intervals, due to variations in the rate of the drum, is obviously no source of error, since the graphic and dial records are simultaneous readings of the same event.

On p. 63 it was, through inadvertence, stated that the effect of the lever on the readings was to "increase (or diminish)" them by about 1σ. The actual effect is to increase them by this amount under all circumstances, the additional weight accelerating the descent of the armature, and retarding its ascent; thus shortening the latency of start, and prolonging the latency of arrest of the hands.

TABLE II.

Showing effect of armature-lever on chronoscope readings.

	Groups of ten readings, without lever		Corresponding groups of ten readings, with lever	
	M.	M.V.	M.	M.V.
i.	98·9 σ	·54 σ	99·9 σ	·72 σ
ii.	99·0	·80	100·8	·62
iii.	238·8	·84	239·4	·96

The average increase in the readings due to the lever is thus ·93σ.

In brief, our further experience of the adjustments, accuracy, and control of the chronoscope, whilst confirming our earlier statements as to the adjustments and accuracy of the instrument, leads us to prefer direct control by its own armature-lever in conjunction with drum keys, to indirect control by other electromagnetic apparatus in combination with the hammer.

THE TRANSFER OF IMPROVEMENT IN MEMORY IN SCHOOL-CHILDREN.

By W. H. WINCH.

I. The problem stated; its bearing on educational theory. II. First series of experiments. (a) Method of procedure, with memory tests in history and practice exercises in poetry. (b) Results of tests, individual and summarised. III. Second series of experiments. (a) Method, with memory tests in geography. (b) Summarised results. IV. Third series of experiments. (a) Method, with memory tests in history. (b) Summarised results. V. Suggested conclusion, that improvement in memory gained through practice in one subject of instruction is transferred to another.

I. THE educational theory current in England some twenty or thirty years ago furnished easy solutions to all questions of this kind. Mental life, according to this theory, was a bundle of faculties, such as Observation, Memory and Imagination. A boy taught to observe the sizes, shapes and colours of material objects would, it was asserted, grow up an observant man: fairy tales would cultivate *the* imagination: and so on.

The theory advanced to-day has rightly introduced very serious limitations to this view, and tends toward the opinion that the results of formal training of any kind are not transferable from one field of mental activity to another.

A solution, it is beginning to be agreed, must be reached, if at all, by means of direct experiment. It may be found that the questions raised are extremely complex and that a number of particular solutions will be arrived at, rather than a general one covering the whole field.

The following work was done to help toward an answer to one of the questions involved in the controversy outlined above, viz. Is the improvement in memory, gained through practice in one subject of instruction, transferred to another subject which has not been practised?

For example, will children practised in learning poetry learn another subject, such as history or geography, more readily because of their improvement through practice in learning poetry?

II. (a) A first series of experiments was carried out in the spring of this year. The pupils selected to do the work ranged in age from 11 years 2 months to 15 years; their average age being 13 years. They comprised Standards VI and VII, forming the first class of a London elementary school for girls situated in a good neighbourhood.

The first step was to divide the children into two groups of equal ability as to memory. This was done, partly on an actual test, and partly on the opinion of the teacher of the class, who had a quite exceptional knowledge of the capabilities of her pupils.

The test set was a passage from a historical reading-book, which was not in the ordinary way accessible to the children of this class. Ten minutes were allowed for memorizing; the work was mainly visual, articulation, however, being permitted, provided that it was not audible. The girls were then required to reproduce in writing as much as they could remember, fifteen minutes being allowed for this.

I thought, first, of marking the exercises in a complicated way, allowing so many marks for the subjects and predicates of the sentences, so many for the adjectival and adverbial phrases and clauses, and so on; but it soon became evident that a much simpler method would be quite satisfactory for the purposes of this experiment. One mark was, therefore, allowed for each word rightly remembered and correctly placed. There were ninety-eight words in the exercise, which is reproduced below.

With the aid of the teacher, the girls were now placed in two equal groups. This was not easy to arrange, but, finally, we succeeded in getting two groups, A and B, which had an equal aggregate of marks and which were thought by the teacher to be equal. The marks obtained in this "Preliminary Test" will be found in Table I.

The members of the A group, during the next week or two, were practised in learning poetry, the B group meanwhile working sums. With this exception, the school work of the two sections was the same during the progress of the experiment. The memory work set, both in history and poetry, is given below.

After four practice exercises had been worked by group A, the two groups were placed together and a final test given in history. The time allowed for each test and exercise and the method of marking were the same in all cases.

The following chronological account may help to make clear exactly what was done.

Preliminary test. 10 a.m. Friday, April 4th.

All the pupils in the class learnt the following:

> "Thus the beginning of Henry's reign is marked by a statute 'for the burning of Heretics,' and directly after, a Lollard named William Sawtre was sent to the stake. In Henry V's reign the Lollards were still numerous enough to threaten a rebellion. They were protected and encouraged by Sir John Oldcastle, a brave soldier who had fought well in Henry IV's wars against the Welsh. He was arrested and sentenced to be burnt, but he escaped. A plot was formed for a great mass of Lollards to meet in St Giles's fields, and to seize the king." (98 words.)

First practice exercise. 10 a.m. Tuesday, April 16th.

Group A memorized 18 lines from Tennyson's "Lady of Shalott," commencing "On either side the river lie." (99 words.)

Second practice exercise. 10 a.m. Friday, April 19th.

Group A memorized 18 lines from Southey's "The Scholar," commencing "My days among the dead are past." (107 words.)

Third practice exercise. 10 a.m. Tuesday, April 23rd.

Group A memorized 18 lines from Byron's poem commencing "She walks in beauty like the night." (120 words.)

Fourth practice exercise. 10 a.m. Friday, April 26th.

Group A memorized 20 lines from Thomas Hood's "Ruth," commencing "She stood breast high amid the corn." (124 words.)

Final test. 10 a.m. Tuesday, April 30th.

All the pupils of both Groups A and B memorized the following:

> "The secret of this is that, during the eighteenth century, Britain became the workshop of the world. We grew rich by our trade and industry. Napoleon was right when he saw that if he could cripple our trade he might conquer us. But our trade was too vast to be crushed by even Napoleon's resources.
>
> At the beginning of the eighteenth century Britain was still chiefly an agricultural country. Her iron trade languished because men had not learned to use coal to work it, and the supply of charcoal from the forests was running short. The linen business was small, chiefly centred in Scotland and the north of Ireland. No true cottons were made, because British spinners could not spin a cotton yarn strong enough for use as warp. The woollen trade was old and fairly vigorous." (137 words.)

It will be seen that the exercises increased in length as they went on, the time for memorizing remaining constant. To show the progress made in learning poetry, the results of the first and fourth exercises are given in the table. Whilst the two sections were at work in the final test, it was noticed that the practised group were working faster; many of these children were apparently satisfied that they knew the passage before the time for memorizing had expired.

II. (b) The following table of marks shows the individual results of the work for both groups.

TABLE I.

GROUP A.					GROUP B.		
Name Initials only	Preliminary history test, max. 98	First practice exercise, Poetry, max. 99	Fourth practice exercise, Poetry, max. 124	Final history test, max. 137	Name Initials only	Preliminary history test, max. 98	Final history test, max. 137
R. D. ...	98	99	123	135	C. D....	98	127
B. G. ...	98	89	117	129	P. E. ..	98	135
S. E. ...	98	81	120	135	L. M....	97	132
F. K. ...	97	98	119	134	B. H....	97	120
F. D. ...	97	89	119	124	T. M....	97	88
K. S. ...	97	99	111	128	K. M....	96	137
K. G. ...	96	97	117	132	T. R....	95	127
D. K. ...	95	99	121	135	W. K....	95	126
P. K. ...	94	62	99	116	G. L....	95	118
B. L. ...	91	59	75	130	C. M....	92	96
T. D. ...	91	77	116	115	M. A....	91	110
S. B. ...	90	70	75	132	B. H....	90	129
H. D. ...	84	75	91	116	C. J. ...	87	102
L. E. ...	80	42	98	111	T. K....	83	82
E. K. ...	72	40	46	100	T. L....	72	94
M. M....	66	54	55	97	A. D....	60	74
G. E. ...	53	68	82	86	S. R....	54	93
Total ...	1497	—	—	2055		1497	1890

As far as possible, the girls in the two groups have been arranged, both in the order of their merit in the preliminary history test, and so that the pupils on the same horizontal lines in the two groups are approximately equal to each other.

A glance at the table will show that Group A, the practised group, is much superior to Group B in the final history test; though, in the preliminary history test, exactly the same aggregate of marks was obtained by the two groups.

Further, a careful scrutiny will show that, in nearly every case, a girl in Group A, who is level with one in Group B in the preliminary test, is superior to her in the final test. There are only two exceptions; thus, P. E. in Group B has 135 marks in the final test against B. G. in Group A, who has 129, though each of them have 98 in the preliminary test; and K. M. in Group B, who started level with K. G. in Group A, is ahead of that pupil in the final test. But there is no other case in which a member of Group A does not become superior to the member of Group B with whom she started on equal terms; though S. R. in Group B, starting only one mark above G. E. in Group A, is more than proportionally superior in the final test.

The general result may, however, be more clearly indicated in the following summary.

TABLE II.

Showing the pupils arranged in sections according to the marks they obtained in the preliminary test in history.

	GROUP A			GROUP B		
Marks obtained in the Preliminary test	Number of children	Preliminary history test, average mark	Final history test, average mark	Number of children	Preliminary history test, average mark	Final history test, average mark
Full marks	3	98·0	138·0	2	98·0	131·0
95—98	5	96·4	130·6	7	96·0	121·1
90—95	4	91·5	123·2	3	91·0	111·6
80—90	2	82·0	113·5	2	85·0	92·0
Below 80	3	63·6	94·3	3	62·0	87·0

This table shows, in a convenient form, the superiority of the sections of Group A over the corresponding sections of Group B in the final test; and is inserted here partly to make this clearer and partly because, for the next series of experiments, summaries only will be given; and it is of advantage to be able to see, at least in one case, the relation between the summary and the individual results.

The clearness of the result I attribute largely to the teacher's assistance in the preliminary grouping, for the results of one test are not in themselves quite adequate as a basis for the division into two equal groups.

III. (*a*) A second series of experiments was made in a girls' school in a poor neighbourhood, with children in Standards VI and VII, ranging in age from 11 years 11 months to 14 years 2 months, with an average age of 13 years 3 months.

The whole class, as in the previous school, was divided into two approximately equal groups, and one was practised in memory exercises, the other not. The preliminary and final tests were, however, exercises in geography instead of history as in the former school; and the poetical extracts given were simpler in meaning. Group B was occupied in writing whilst Group A was memorizing. The time allowed for memorizing and the method of marking were as before.

A brief chronology will show the exact course of the experiments.

Preliminary test. 10.50 a.m. Tuesday, May 28th.

All the class memorized the following passage from a geographical reading-book:

> "One day we went to the Tower of London, a fortress built hundreds of years ago on the left bank of the Thames. In the centre is a large building called the White Tower, and around that are walls and towers. Some of the towers have been the prisons of kings and queens and other great persons.
>
> In the White Tower are thousands of guns, swords, and other weapons of war, and the axe and block that were used when persons were beheaded.
>
> In another tower we saw the King's crown." (91 words.)

On the results of this work the class was divided into Groups A and B.

First practice exercise. 10.50 a.m. Friday, May 31st.

Group A memorized 15 lines from Mrs Hemans' "Prayer at sea after victory."

Second practice exercise. 10.50 a.m. Tuesday, June 4th.

Group A memorized 16 lines from Mrs Hemans' "Cœur de Lion at his Father's Bier."

Third practice exercise. 10.50 a.m. Friday, June 7th.

Group A memorized 12 lines from Mrs Hemans' "Indian with his dead child."

Fourth practice exercise. 10.50 a.m. Tuesday, June 11th.

Group A memorized 16 lines from Mrs Hemans' "Ivan the Czar."

Final test. 10.50 a.m. Friday, June 14th.

All the class, consisting of both Groups A and B, memorized the following passage from a geographical reading-book:

> "Cambridge stands in a flat country by the side of the river Cam, but the roads leading to Oxford are very hilly, and from them a beautiful view is obtained of the city, with the spires, domes and towers of the colleges and churches.
>
> There are nearly twenty colleges at Cambridge, and Oxford also has a great number. The chief buildings in both places are so beautiful that many persons go on purpose to look at them.
>
> The noblest college in Cambridge is known as Trinity College. It is between three and four hundred years old, having been founded by King Henry the Eighth." (104 words.)

III. (*b*) It will probably be sufficient in this case to give the results in a summarised form.

TABLE III.

Showing the pupils arranged in sections according to the marks they obtained in the preliminary test in geography.

	GROUP A			GROUP B		
Marks obtained in the Preliminary test	Number of children	Preliminary geography test, average mark	Final geography test, average mark	Number of children	Preliminary geography test, average mark	Final geography test, average mark
90 and over	8	90·3	98·6	3	90·8	95·0
85—90	6	88·0	92·0	6	88·2	98·1
75—85	5	80·0	94·6	5	80·4	85·2
Below 75	8	71·6	81·0	3	71·0	61·6

Comparing the corresponding sections in Groups A and B there is decided superiority in the final test of the practised over the unpractised group, except in the second section, viz. those obtaining 85 to 90 marks. This was due, however, to one child in Group A who, after scoring 88 marks in the preliminary test and working four practice exercises with continuous improvement, *dropped* to 75 marks in the final test in geography. Had she been omitted the average of this section of Group A would have been 95·4.

IV. (*a*) A third series of experiments was carried out in an elementary girls' school intermediate in neighbourhood between the first and second. The whole of the first class did the work, and, as the children had only just entered their present standards, their level of general attainments would be most accurately described as that of Standards V and VI. These children ranged in age from 10 years 9 months to 14 years, their average age being 12 years 8 months.

The general scheme of tests and exercises resembled that of the two previous experiments. The preliminary test was a historical passage, as in the first school. The practice exercises were pieces of poetry, as in the two preceding schools. Ten minutes were, as usual, allowed for memorizing, but an indefinite time was given for writing the passage afterwards. There was, moreover, a slight variation in this case in the marking of the results. As before, one mark was allowed for each word remembered correctly in its proper place; but, from the total thus obtained, a mark was subtracted for every word omitted, inserted, or substituted within the sentences remembered.

Whilst Group A was memorizing poetry, Group B was occupied with ordinary routine work in arithmetic.

On the morning when the two groups, A and B, were again placed together, and were working what was intended to be their "final history test," His Majesty's Inspector visited the school. He, in consonance with the present attitude of the Board of Education towards properly conducted experiments in pedagogy, cordially entered into the spirit of the work. But, as may readily be supposed, there was a probability of some distraction, so that it was decided at once to pursue the experiment further. Two more exercises in poetry were given to Group A and a further history test was given to the whole class a week or so later.

The sequence of the work was as follows:

Preliminary test. 10 a.m. Friday, Sept. 13th.

All the children memorized this passage from a historical reading-book:

> "Only sixteen months elapsed between the signing of Magna Charta and the death of John; but his conduct during that short time was enough to turn all hearts from him.
>
> No sooner were the troops of the barons dismissed than the king, at the head of his hired foreign soldiers, proceeded to lay waste his own country. From Dover to Berwick he passed, burning, slaying, torturing.
>
> The result was that some of the barons asked Louis, the son of the French king, to come over and take the English crown. Louis landed upon the Kentish coast and marched straight for London. Soon afterwards news came of John's death." (108 words.)

First practice exercise. 10 a.m. Tuesday, Sept. 17th.

Group A memorized 14 lines from Eliza Cook's "King Bruce of Scotland."

Second practice exercise. 10 a.m. Friday, Sept. 20th.

Group A memorized 16 lines of Mrs Hemans' poem commencing "They grew in beauty side by side."

Third practice exercise. 10 a.m. Tuesday, Sept. 24th.

Group A memorized 18 lines from Coleridge's "Ancient Mariner."

Fourth practice exercise. 10 a.m. Friday, Sept. 27th.

Group A memorized 18 lines from an anonymous author, "How to build a nest."

Second test in history. 10 a.m. Tuesday, Oct. 1st.

Groups A and B memorized the following:

"But again the black shadow of war hung over the land!

It was caused by a quarrel between the Russians and the Turks. Great Britain and France sided with Turkey.

Turkey was a weak and decaying power. It was known as 'The Sick Man'—'a man sick unto death,' as the Czar himself said.

Russia on the other hand was very powerful, and it was thought unsafe for the other nations of Europe that she should increase her power.

But Britain had special reasons for interfering in the quarrel. It was believed that Russia aimed at the capture of India, and that her attack on Turkey was but the first step in a great plan against Britain." (117 words.)

Fifth practice exercise. 10 a.m. Friday, Oct. 4th.

Group A memorized 16 lines by Mrs Hemans about the Pilgrim Fathers.

Sixth practice exercise. 10 a.m. Tuesday, Oct. 8th.

Group A memorized 18 lines of Southey's "Old Kaspar."

Final test in history. 10 a.m. Friday, Oct. 11th.

Groups A and B memorized the following:

"Of later British leaders, perhaps the most loved was the good General Gordon. He fought during the Crimean War, after which he went to China. There he subdued the wild bands of rebels who had been the terror of the people.

Soon after, Gordon was appointed Governor-General of the Soudan, a part of Africa subject to Egypt. The country was in a very sad and unsettled state and a most cruel slave trade was carried on by Arab traders. Gordon did his work well. Never before had the Soudan been so peaceful and prosperous.

In after years, when war again broke out among the wild tribes, Gordon returned to the Soudan to try to restore peace." (117 words.)

In the preliminary history test the children had already some acquaintance with the facts, but, in the second and third, they had no knowledge of any kind.

IV. (*b*) The results of the tests in history follow:

TABLE IV.

Showing the pupils grouped in sections according to the marks they obtained in the preliminary test in history.

	GROUP A				GROUP B			
Marks obtained in the Preliminary test	Number of children	Preliminary history test, average mark	Second history test, average mark	Final history test, average mark	Number of children	Preliminary history test, average mark	Second history test, average mark	Final history test, average mark
98—108	7	103·8	104·9	104·6	7	102·6	100·6	100·6
68—98	8	84·5	89·5	77·3	8	82·5	80·3	64·8
48—68	8	58·3	69·8	64·0	7	58·8	65·8	46·7
0—48	4	28·5	68·2	64·7	5	27·4	50·5	41·6

It will be seen that, in every case, the practised group do better work in the second and final history tests than the corresponding non-practised group.

I have given the historical passages to be memorized, partly because, from the results, they appear to increase in difficulty. Otherwise it would seem that some of the groups fall back as the exercises proceed. If this were so, it would be contrary to the general rule. It is not of importance for the question here dealt with, which is settled by the results of corresponding sections in the same work; but it seemed to me worth mention. It would, of course, be possible to try independently, with equal groups of unpractised children, whether the third history exercise is more difficult than the second: the first history test, unfortunately, dealt, as I have said, with a subject with which the girls were somewhat acquainted.

V. The conclusion seems definite and clear. Improvement, gained by practice in memorizing one subject of instruction, is transferred to memory work in other subjects whose nature is certainly diverse from that in which the improvement was gained.

This, at least, is true as far as children of these ages and attainments are concerned. I expressly add this limitation as to age, for inferences from adult psychology to child psychology and to pedagogical practice are extremely unsafe, and I am anxious to avoid the opposite error.

Perhaps, too, I may add, to prevent misunderstanding among teachers, that I am not advocating that children shall learn their geography and history *by rote*; this is done here for experimental purposes. The results do appear, however, to strengthen the case of those who wish children to learn much poetry; it is not an obstacle, but an aid, to the acquisition of other knowledge. It is usually thought, however, that a memory for substance is more important than rote-memory, and it is not shown here that learning poetry strengthens that. The relation between rote-memory and substance-memory in school-children I reserve for later consideration.

THE EFFECT OF PRACTICE ON THE PERCEPTION OF THE MÜLLER-LYER ILLUSION.

By E. O. LEWIS.

(*From the Psychological Laboratory, Cambridge.*)

I. *Aim of the experiment: to determine quantitatively the amount of the illusion when the figures are exposed for prolonged and momentary periods; and to find the influence of practice in the momentary and prolonged observations.*

II. *Apparatus and Method.*

III. *Readings in tabular form, and diagrammatically represented.*

IV. *Results: The illusion greater by momentary than prolonged observation. Disappearance of illusion with practice in the prolonged series, but momentary series not influenced by practice.*

V. *Theoretical Conclusions.*

I. The Müller-Lyer Illusion has had its fair amount of attention from psychologists during the last twenty years. Consequently one feels it necessary to give reasons justifying further investigation of this illusion. First, the method of adjustment, used by almost all the persons who have hitherto determined the amount of this illusion, does not yield results sufficiently accurate; and secondly, in view of those explanations of this illusion, which are based upon the movements of the eyes, we naturally expect interesting results when the figures are observed momentarily. This paper will deal primarily with the influence of practice on the amount of this illusion when the figures are observed for prolonged and momentary periods.

II. *Apparatus and Method.*

The same apparatus and method were used in these experiments as described in the preceding paper on "The Illusion of Compared Horizontal and Vertical Lines" (Hicks and Rivers)[1]; and hence it is unnecessary to repeat the description of them in this paper. Moreover some of the results obtained by the experiments which are dealt with in the present paper merely verify what had been already discovered during the previous experiments which have been mentioned. When this is the case the results will be simply stated, and the reader is referred to the preceding paper for the treatment of their significance.

---

[1] This volume, p. 243.

It is necessary to add a few remarks concerning the two plates used for obtaining the figures on the screen during the course of these experiments. Plate A had slits cut in it so as to form the Brentano Figure (Fig. 1); whereas Plate B had the slits arranged as a figure in Fig. 2.

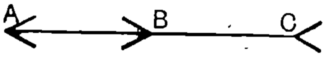


FIG. 1.

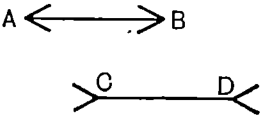


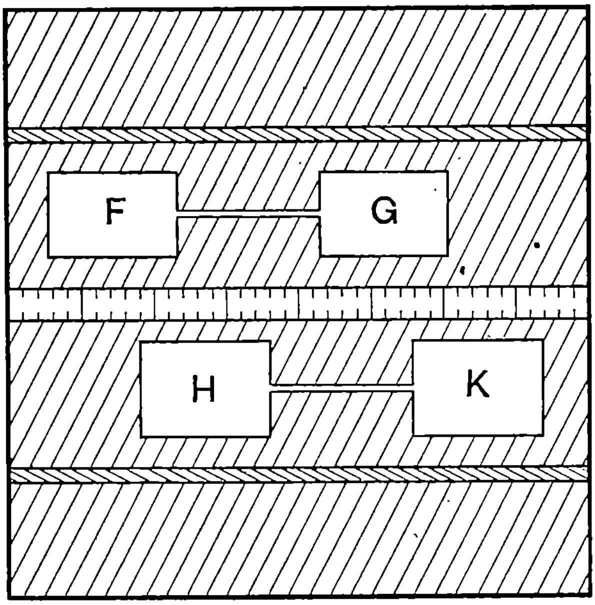
FIG. 2.

In both figures the oblique lines were 10 mm. long and formed an angle of 36° with the middle lines.

In Plate A, the line *AB* was a fixed length (50 mm.) and could not be varied; but *BC* was variable. The subjects found that with this plate there was a tendency, especially in the momentary observations, to remember the length of the constant line *AB*, and therefore to pay attention exclusively to *BC*. That this did not occur to any appreciable degree was proved by exposing *BC* alone, and the readings, when given, were erratic and inconsistent, as compared with those given when both lines were exposed. As in the previous experiments there was a tendency to compare the lines exposed at any one moment with those which preceded them. This was obviated with Plate B by varying both lines indiscriminately. Fig. 3 represents Plate B, and one of the slides with an arrow-head slit. The open spaces, *F*, *G*, *H*, *K* were covered by the slides which moved along the grooves. The width of the slits in Plate B were such as to give lines 1 mm. thickness on the screen *F*, whereas with Plate A the slits gave lines 2 mm. in thickness. The standard *AB* in Fig. 1 was 50 mm. long. In Fig. 2 it was found convenient to make *AB* 50 mm. long when standard, and *CD* 40 mm. long. The variable in both plates was moved through a range of 5 mm. With Plate A, ten readings were taken of each setting, which made a total of 100 readings each day by each observer. On account of the double standard with Plate B only five readings were taken of each setting, which gave the same total number as with Plate A. The length of the lines exposed was measured by a scale on the middle of the plates.

The method of obtaining readings was that of right and wrong cases. This method is more accurate than the method of adjustment, and less fatiguing for the subject. There is one difference between the gradation of the readings in these experiments and that adopted in the previous experiments referred to. In the latter the readings were graded as "distinctly longer," "definitely longer," "indefinitely longer," "no difference," "indefinitely shorter," "definitely shorter," "distinctly shorter." At first the same gradation was adopted in these experiments; but after taking readings for a few days the number given as "distinctly longer" or "distinctly shorter" was so few that it was decided to include these with the "*definite*" readings, and consequently the gradation was reduced to "definitely longer," "indefinitely longer," "no difference," "indefinitely shorter," "definitely shorter." It must be admitted that this change was made somewhat prematurely; and had the change not been made, it is probable that the number of 'distinct' readings would have increased with practice.

PLATE B



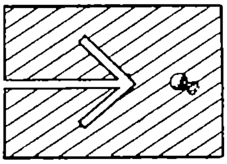


FIG. 8. Slide with arrow-head.

III. *Results tabulated, and represented diagrammatically.*

Observations were made by six different persons, one of whom gave readings with both plates[1]. Three series were made with Plate A, and four series with Plate B. Only two of the subjects, namely, D. J. and R. R., were unacquainted with the nature of the illusion until they had finished their series; but none of the observers, except the experimenter himself, knew what was expected to be the influence of practice. Only with one subject (R. R.) have readings been given without a single day's pause. A day's pause is represented in the following tables by an asterisk set between the numbers indicating the amount of the illusion; and two asterisks represent a two days' pause.

It has been found that the results are best presented by giving the amount of illusion in millimetres for each day. With Plate B, there were two sets of readings, one for each standard. In the following tables the mean of these readings is given, when they have been found to differ. The numbers in the columns under the letters M. and P. denote the amount of the illusion by momentary and prolonged observation respectively.

TABLE I.

Readings with Plate A. Standard AB, 50 mm.

	E. O. L.		R. F. H.		A. J. J.	
Days	M.	P.	M.	P.	M.	P.
1	11·5	7·5	12·5	8	12	9·5
2	12	7·5	11·5	7	11	10
3	11	8	13	7	11	10
4	12	8	12	7	11	10·5
5	* 11	7·5	12·5	6·5	* 12	9
6	11·5	8·5	11	6·5	11	10
7	* 11·5	7	* 11	5·5	12	8
8	11·5	7	11	5·5	12	7
9	11·5	6·5	* 11	5	12	7
10	11·5	7·5	11	4·5	12	7
11	11·5	8	12	4·5	* 11	5
12	* 11·5	7	11	4	11	5
13	* 11·5	6	11	4	10·5	4·5
14	* 11·5	5	* 12	3	* 10	2
15	* 10·5	4	12	2	10	2
16	* 11·5	3	** 11	1·5	10	2
17	* 10	2	11	·5	10	2
18	10	2	11	0	10	0
19	9	1·5				
20	9	0				

---

[1] Some time was allowed to elapse between the two series of observations by this subject, so as to minimise the influence of practice which, as shown later, tends to be transferred.

TABLE II.

Readings with Plate B. Standards AB 50 *mm., CD* 40 *mm.*

	E. O. L.		B. F. B.		R. R.		D. J.	
Days	M.	P.	M.	P.	M.	P.	M.	P.
1	11	8	8·5	7	13·5	9	7	4
2	9	8	7·5	6	14	7	8	5
3	9·5	8	* 7·5	4	13	6	7	5
4	10	7	** 7·5	4	12	5	* 7	3·5
5	12	7	** 7·5	4	12	5	* 8	1·5
6	* 12	7	8·5	4	11	4	9	1
7	12·5	7	* 8	3	11	4	9	·5
8	12·5	7	9	2	11	3	9	0
9	11	7	8·5	2	12	2		
10	* 11	6	9	2	10	2		
11	11	6	9	1	10	1		
12	11	5	* 9	0	10	0		
13	* 10	3						
14	10	2						
15	10	2						
16	9	1						
17	10	·5						
18	11	0						

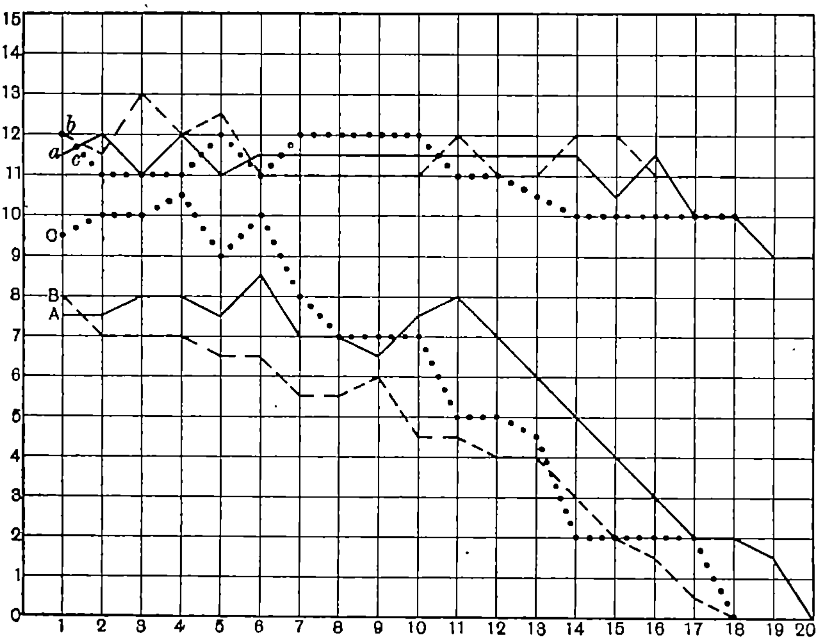


FIG. 4.

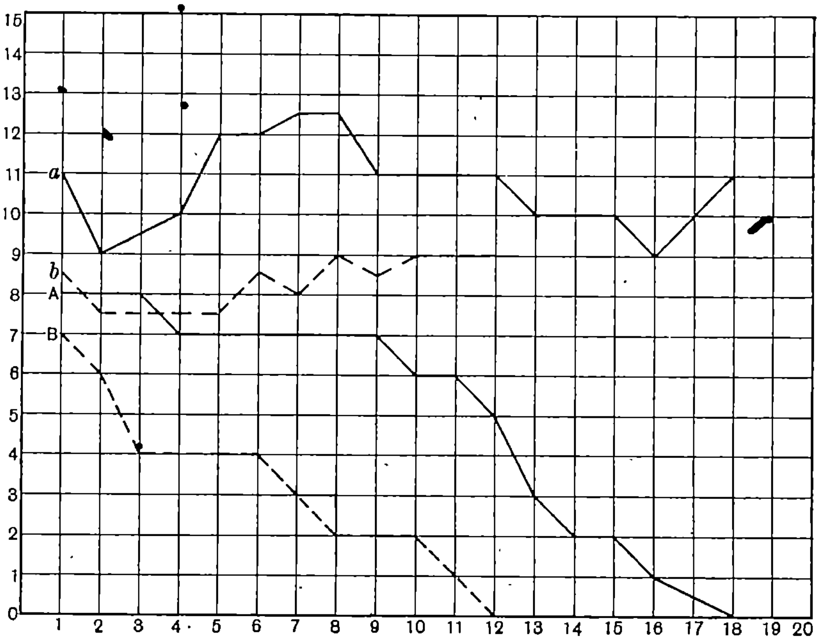

0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
a
b
A
B
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20

FIG. 5.

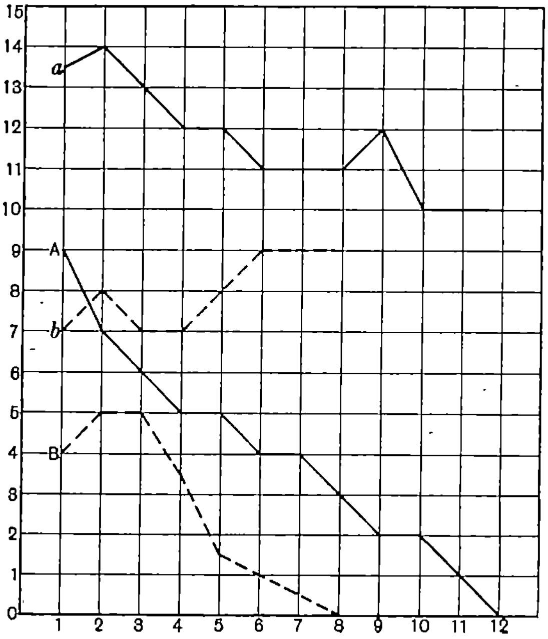

0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
a
A
b
B
1
2
3
4
5
6
7
8
9
10
11
12

FIG. 6.

DESCRIPTION OF FIGURES.

The horizontal numbers indicate the days on which readings were taken; and the vertical numbers the amount of the illusion on each day.

FIG. 4.	Curve	*a*	represents the	momentary	readings of	E. O. L.	with Plate A.
	,,	*A*	,,	prolonged	,,	,,	,,
	,,	*b*	,,	momentary	,,	R. F. H.	,,
	,,	*B*	,,	prolonged	,,	,,	,,
	,,	*c*	,,	momentary	,,	A. J. J.	,,
	,,	*C*	,,	prolonged	,,	,,	,,
FIG. 5.	Curve	*a*	represents the	momentary	readings of	E. O. L.	with Plate B.
	,,	*A*	,,	prolonged	,,	,,	,,
	,,	*b*	,,	momentary	,,	B. F. B.	,,
	,,	*B*	,,	prolonged	,,	,,	,,
FIG. 6.	Curve	*a*	represents the	momentary	readings of	R. R.	with Plate B.
	,,	*A*	,,	prolonged	,,	,,	,,
	,,	*b*	,,	momentary	,,	D. J.	,,
	,,	*B*	,,	prolonged	,,	,,	,,

IV. *Results obtained.*

It is evident from the above results that the momentary and prolonged observations differ definitely in one respect. The effect of practice on the momentary series is ambiguous. The three series with Plate A show a tendency for the amount of the illusion to decrease, and so does the series of R. R. with Plate B; but the series of D. J. and B. F. B. with Plate B show a tendency to an increase of the illusion, whilst in the series of E. O. L. with Plate B the illusion is the same on the last day as it was on the first. On the other hand the influence of practice in the prolonged series is perfectly definite. In each series the illusion diminishes and ultimately disappears completely. It is a significant fact that the subjects were not aware of any such change with practice. When at the end of the series they were shown the two lines which at the beginning they had *seen* as equal, it seemed incredible to them. The only difference effected by practice which could be detected by introspection, was the overcoming of the distraction caused by the presence of the oblique lines. With practice the lines to be compared became more prominent than the other portions of the figures. However, the subjects thought that they had completely overcome the distracting influence of these oblique lines, when, in fact, the illusion had but half disappeared. This proves that the illusion does not disappear when the subject ceases to exert effort to overcome the distraction caused by the oblique lines. Voluntary effort cannot accomplish what practice does.

It is also interesting to find what influence previous knowledge of the illusion and of the effects of practice has upon the readings. Such knowledge one would expect to increase the effects of practice in the prolonged series. The above readings give no proof of any such influence. The experimenter E. O. L., who alone knew what other persons had found to be the effects of practice, required the greatest number of readings before the illusion disappeared; whereas with D. J., who was not even acquainted with the nature of the illusion, it disappeared in the shortest time.

The decrease of the illusion in the prolonged series was more or less continuous, although the diagrams show much irregularity. There is evidence in the results that the regularity of decrease depends on the regularity of the days on which readings were made. For instance, the most regular diagram of prolonged readings is that of R. R. (Fig. 6, line *A*), and he was the only person who gave the whole series without a day's pause. The influence of one day's pause was to accelerate the rate of decrease, which agrees with what Professor Judd found[1]. A remarkable instance of this occurs in the series of E. O. L. (Fig. 4, line *A*). The exceptional rapid and regular decrease which took place from the 11th to the 17th days corresponds to the fact that all these readings were taken on alternate days. The other readings also illustrate the accelerating of the rate of decrease by pauses of one day. There is evidence that when a pause of two days was taken the effect of the pause was neutralised by the loss of practice. This we see in the readings of B. F. B. (Fig. 5, line *B*), as there was a two days' pause between the 3rd and 4th readings and between the 4th and 5th.

When the illusion had disappeared with the prolonged exposure, another series of observations were made with the plates inverted, so that in the case of the image given by Plate A the line *AB* was to the right and *BC* to the left; and similarly with Plate B the lines interchanged positions. The following table gives the results obtained by this procedure. Column A gives the amount of illusion at the beginning of the experiments, and the numbers in brackets indicate the amount of the illusion when the first series was completed. Column B gives the amount of the illusion with the inverted plate immediately after completing the first series.

---

[1] *Psychological Review*, Vol. IX. p. 27.

TABLE III.

Plate	Subject	Prolonged		Momentary	
		A	B	A	B
Plate A	E. O. L.......	7·5 (0)	6	11·5 (9)	8·5
	R. F. H.......	8 (0)	5	12 (11)	10·5
	A. J. J.	9·5 (0)	6	12 (10)	11
Plate B	R. R.	9 (0)	3·5	13·5 (10)	10
	D. J.	4 (0)	3	7 (9)	8·5
	E. O. L.......	8 (0)	5	11 (11)	10
	B. F. B......	7 (0)	5	8·5 (9)	10

With three of the above subjects the observations were continued with the inverted plates until the illusion again disappeared in the prolonged observations. Table IV gives the results of these series, the numbers indicating the amount of illusion on each successive day. All three series were taken with Plate B.

TABLE IV.

Subject	Prolonged					Momentary				
E. O. L. ...	5	2	1·5	·5	0	10	10	10	9·5	10
R. R..........	3·5	1	·5	0	—	10	10	10	10	—
D. J.	3	2·5	·5	0	—	8·5	8·5	9	9	—

Table III shows that there has been a 'transference' of the effect of practice; or, more precisely, the disappearance of the illusion as the result of practice in observing one figure, involves the diminution of the amount of the illusion when similar figures are observed. In the prolonged series the illusion returns, but not to its original amount. In the momentary series the first readings of the new series are practically the same as the last momentary readings of the first series. Table IV makes still more evident the 'transference' of the effects of practice, as the illusion diminishes very rapidly in the new series. The subject E. O. L. gave readings with both plates, and the fact that this could be done gives some idea of the duration of the effects of practice. About a week elapsed between the two series made by this subject, and it is evident that by this time the effects of practice in the first series had but little influence, if any, on the second series, although it must be borne in mind that the figures were different ones in the two series.

Another important result obtained by these experiments is the relative amount of the illusion with momentary and prolonged observation. In no single case do the lines representing the momentary and prolonged readings intersect. The readings on the first day of each series show that for these six subjects the 'momentary' illusion is on an average 3·5 mm. greater than the 'prolonged' illusion. Several other subjects were examined, and without exception they verified what has just been stated. This result in itself is of great theoretical interest, as will be seen presently.

TABLE V.

Subject	Prolonged			Momentary		
	Definite	Indefinite	No difference	Definite	Indefinite	No difference
E. O. L. ...	354	497	99	596	324	30
R. F. H. ...	365	358	27	383	350	17
A. J. J.	346	536	18	505	389	6
R. R.	213	325	62	319	261	20
D. J.	180	212	8	219	176	5
B. F. B. ...	158	342	100	275	282	43
E. O. L. ...	319	480	101	491	358	51
Total.........	1935	2750	415	2788	2140	172

These experiments verify completely the remarks in the preceding paper as to the relative definiteness of the momentary and prolonged observations. Naturally, a little difficulty was experienced at first in fixating the eyes properly for the momentary exposure; but once this was overcome, the momentary readings were decisive and consistent; whereas with the prolonged observations the subject oscillated between two judgments, and the readings were less consistent. During the later stages of the momentary series a range of 3 mm. for the variable would have sufficed to determine the amount of the illusion, whereas with the prolonged series, the range of 5 mm. was barely enough. One of the subjects, who was unacquainted with the method of varying the lines, remarked during the course of the experiment that he found it easier to judge the momentary series because the lines were varied more than in the prolonged series. In fact the amount of variation was the same, and his remark proved that his discrimination of slight differences was better in the momentary series than in the prolonged.

As final evidence of the superior definiteness of the momentary observations, the readings obtained, over ten thousand in all, have been tabulated in Table V as was done in Table XVII of the preceding paper, with the difference that no distinction has been made in the present table between '*distinct*' and '*definite*' readings.

V. *Theoretical Conclusions.*

As mentioned before, it is not intended to discuss in this paper the explanations given of the Müller-Lyer illusion. Yet the above results are so significant in relation to the influence which movements of the eyes have been supposed to exert in the production of the illusion, that it is impossible to overlook the point in this paper. In these experiments the figures were observed when the eye was given sufficient time to move along the lines, and also when it had no time to make any appreciable movement. With the latter kind of observation, the illusion was decidedly greater than with the former. This seems a conclusive disproof of explanations of this illusion, such as Wundt's, which attribute it to the free and restricted movements of the eyes. That movements of the eyes have a subsidiary bearing upon this illusion is quite possible; but in the light of these experiments another explanation of it must be sought.

Professor Judd has emphasised the importance to the genetic study of perception of results obtained from experiments dealing with the effects of practice. Not only are the above results interesting as showing the influence of practice, but also they have a bearing upon the vexed problem of the function of eye-movements in the development of our space-perception. The momentary observations correspond to the earlier or naive form of perception and the prolonged observations to the later stage of perception. The greater decisiveness and sensitiveness of the momentary observations prove that movements of the eyes have little to do with the accurate measurement of spatial magnitudes. It is true that by moving the eyes we are able to fix the attention on different parts of the visual field, that is, to bring to the focus what was previously in the periphery of the field or outside it; but when we judge the magnitude of objects already in the focus, movements of the eyes give us no aid. If this view be accepted there is still left the problem as to the function of eye-movements in the development of perception. The influence of practice on the momentary and prolonged observations may yield some light on this point.

It may be assumed that some such explanation as Dr Rivers[1] gives of this illusion brings forward its main feature. The illusion is attributed to the fact that our perception of a part of the figure is influenced by the percept of the whole figure. If this explanation is correct, as we have every reason to believe, the illusion exists because the observer fails to judge one part of the figure without being influenced by the other parts of it. Equally evident is it, that what practice effects is the restriction of attention to the parts which are to be compared. This is confirmed by what the observers discovered by introspection. They remarked that after some practice in observing the figures, they were better able to see the middle lines themselves, and that they overcame the distracting influence of the oblique lines which was present at the beginning of the series. This increased capability for restriction is one important feature distinguishing the higher forms of perception from the lower. As emphasised above, this does not mean increased sensibility of discrimination of magnitude. What it involves is the capability of seeing the part itself, and not merely in relation to the whole. Some time is required to effect this restriction; and this explains why in the momentary observations of these experiments the illusion is not influenced by practice. That the movements of the eyes in the prolonged observations undergo a change with practice, corresponding to the increased restriction, is very probable. Professor Judd has investigated the influence of practice on the movement of the eyes whilst surveying the Müller-Lyer figures[2]. He found that with practice the movements of the eyes become more systematised, and the ends of the lines are fixated more accurately. That there is such correlation between perception and movement is admitted by all. But psychologists differ when it is stated that the development of perception depends on the systematisation of eye-movements. Rather than ascribe the disappearance of the above illusion to the organised movements of the eyes and the better fixation of the ends, it might be attributed primarily to the more efficient restriction of attention to the lines; and as the result of this restriction it is natural that the movements would become more systematic, and the extremities of the lines fixated more accurately. Introspection, so far as it goes, favours the latter view. It might be urged against this view that the illusion was still present, when the subjects thought they had completely effected the restriction of attention. This may be answered by saying that whereas the positive results of introspection are important to psychology, its negative results cannot be relied upon, as the next paragraph proves.

---

[1] *Report of the Cambridge Anthropological Expedition to Torres Straits*, Vol. II. Part 1, p. 126.

[2] *The Psychological Review*, Monograph Supplement, Vol. VII. No. 1, p. 55.

This leads to the last point of this paper. It is a significant fact that none of the subjects were aware of any change in their readings, as the series proceeded. That such a large change in the relative magnitudes of the lines could have occurred, and that introspection gave no indication of it, points to the fact that in solving the problems of genetic psychology it is impossible to rely upon introspection alone. In discussing this unconscious perceptual change, Professor Judd writes: "The whole genetic problem is seen to be one requiring methods which reach beyond introspection, and requiring, furthermore, a careful scrutiny of the qualitative aspects of the changes effected through development."

In conclusion, I wish to thank Dr Rivers for his kindness in allowing me to use the apparatus, and for the valuable aid I have received whilst pursuing these experiments under his direction. I am also indebted to those who kindly consented to become subjects for these experiments.

A NEW VISUAL ILLUSION OF DIRECTION.

By JAMES FRASER,

Deputy Medical Superintendent, Central London Sick Asylum.

I. The new illusory figure consists essentially of 'units of direction.' Properties of a black and white twisted cord. The Simple unit of direction. The Compound unit of direction.

II. The properties of the illusory figure as illustrated by Fig. 1. *The illusion persists under all conditions of examination. Figs.* 1 *and* 2 *compared. The peculiar rôle of the chequer-work background.*

III. The construction described of a triple series of variants of the illusory figure. Properties of these variants.

IV. The construction described of two coloured variants. The properties of the coloured variants. Certain visual properties of red and greenish-blue demonstrated by two experiments with each of the coloured variants.

V. The construction of the circular figures described (with certain properties of some of these figures).

VI. General properties of the circular figures.

VII. An illusory figure examined in relation to a corresponding non-illusory figure.

VIII. The Münsterberg or chequer board illusion as related to the 'twisted cord on chequer-work background' illusion.

IX. The nature of the unit of direction illusion. Certain negative and positive characters indicated.

I.

In all the hitherto published visual illusions of direction, with the exception of the Chequer-board or Münsterberg illusion, the illusory lines or bands are definitely continuous, uninterrupted in character, of black or of white, on a contrasting background. In the illusion of direction here described each illusory band consists of a series of visibly discrete similar parts, all inclined at the same small angle to the line of direction of the series to which they belong. Such visibly discrete similar parts may be conveniently termed 'units of direction.'

Where the illusory band consists of alternating black and white 'units of direction,' it may be conveniently regarded as representing a cord consisting of two strands, black and white, twisted together. This arrangement will, in the descriptions, be referred to as 'the twisted cord.' A twisted cord laid upon a grey or coloured background of intermediate luminosity will apparently deviate from its actual line of direction at an inclination corresponding in trend (but at a smaller angular degree) to the inclination of the units of direction. This illusion is much increased in degree when the twisted cord is laid upon a chequer-work background of squares of white, black, and an intermediately luminous grey or colour in such a way that it bisects diagonally each member of a series of black and white squares, its black and white units bisecting respectively white and black squares (their contrasts) at a small angle with the diagonal line of the square as seen in No. 14, Fig. 15. In such a figure each unit of direction is now, in effect, lengthened by the addition of a triangular area of the same luminosity at each of its ends (No. 14, Fig. 15). These triangular areas are derived or borrowed from the neighbouring squares belonging to the series on which the twisted cord lies, and they lie on opposite sides of the twisted cord. This form of unit of direction may be conveniently (as distinguished from the 'simple unit of direction') termed a 'compound unit of direction,' its middle or rod-shaped portion corresponding to the twisted cord and the triangular end portions corresponding to black or white squares of the chequer-work background. This form of the illusion, in which there is a visual fusion of the twisted cord with a chequer-work background, may be termed the 'twisted cord on chequer-work background' illusion.

In some of the figures the twisted cord must be regarded as fixed at certain points, and, starting from these points, to be twisted in an opposite direction.

In Fig. 2 and in the outer and inner pair of curves in Fig. 6 the strands are not twisted but lie uninterrupted, side by side, their marginal dividing line being the diagonal line of a black or white square. This arrangement, which may be named the 'untwisted strands on chequer-work background,' gives a figure which contains a slight illusory element contrasting with the much more definite illusion of the twisted cord.

II. *The properties of Fig.* 1.

In the following six sections the main properties of Fig. 1, representing the word LIFE, are described. The letters, it should be remarked, are assumed to be seven inches in height. The writer's vision is normal.

(1) The illusion persists through the entire range of visibility of the letters both in relation to the size of the image and to the amount of illumination. In bright diffuse daylight illumination the letters are visible, as such, up to a distance of about 45 feet. Beyond this point the chequered background is vaguely streakily smudged in the areas occupied by the letters. As the figure diminishes in size the letters correspondingly lose their quasi-continuous outline character and are resolved into overlapping black and white fragments (the 'units').

When Fig. 2, similarly constructed, but with letters of really continuous outline ('untwisted strands on chequer-work background') is examined in the same way, the letters remain rectangular and erect under all conditions, but become slightly sinuous or wavy at a distance of 12 to 15 feet. This sinuosity increases with the decrease in size of the image until finally at about 45 feet the letters are represented by a smudging of the chequered background.

The resolution into overlapping units in Fig. 1 and the change from straightness to sinuosity in Fig. 2 seem to be identical visual phenomena. Under the conditions of size and distance stated—in Fig. 1 some point in each triangular area forming the end of a compound unit of direction becomes the definite visual terminal of the unit, and in Fig. 2 the apex of each triangular area attached to the black and white strands becomes the visual terminal of short curves which form a black series and a white series, side by side, the middle points of the curves lying on the marginal dividing line of the black and white strands.

(2) If Fig. 1 be turned through a right angle in the plane of the paper the illusion is sensibly increased for the long limits of the letters, the apparent angular inclination being greater in the horizontal than in the vertical positions.

(3) The illusion is undiminished in degree as perceived during a single flash illumination of a duration of $\frac{1}{8}$ second.

(4) On steady fixation the illusion persists.

If a point be fixated midway between two of the letters, at a distance of eight inches to three feet, the outlined letters and the chequer-work background sensibly compete or alternate with each other in consciousness, the rod-shaped middle portions of the compound units of direction being annulled or swamped, as it were, at time intervals, by the dominance of the sweeping dark and light broad diagonal bands formed by the visual union of the black and white squares with the grey squares. In the negative after-image this alternation can be distinctly perceived, the illusion of direction being present as the letters periodically appear.

(5) If letters outlined by the 'untwisted strands' as in Fig. 2 be rotated round their central points on the chequer-work background so as to correspond in actual angular inclinations with the apparent angular inclinations in Fig. 1, and viewed at a distance of about six feet, this angle is found to be slightly less than the angular inclination of the units of direction.

(6) If Fig. 1 be binocularly combined with a figure of similar size and structure in which there are reversed angular inclinations, unit for unit, the letters directly regarded become erect, and a slight amount of ambiguous relief is obtained. The units of opposing angular inclinations in the vertical limbs will, when so combined, result in units which have no lateral inclination, but seemed to be turned about horizontal axes lying in the plane of the paper. The units in the horizontal series, in combination, are crossed. The letters thus retain their continuity, but appear to be slightly disengaged from the background.

III. *A triple series of variants of Fig.* 1.

By a step by step removal of similar geometrical parts from Fig. 1 a triple series of figures can be obtained of importance for a study of the illusion.

SERIES I. A white background series.

1ª. Removal of all the grey squares.

1ᵇ. Removal of all the black squares with the exception of those forming parts of the black units.

1ᶜ. Removal of the lateral angular parts of these black squares.

1ᵈ. Shortening of the black units so that they no longer overlap but are separated by intervals of the same length as themselves.

1ᵉ. These intervals are filled by similar black units of the same angular inclination so that the units in a series touch each other at angular points.

SERIES II. A corresponding black background series (not illustrated).

SERIES III. A grey background series.

1^{3a}. Removal of all the black and white squares with the exception of those forming parts of the black and white units.

1^{3b}. Removal of the lateral angular parts of the squares of all the units.

1^{3c}. Shortening of the units so that they no longer overlap but touch one another at angular points.

In Series I the illusion diminishes in sharpness up to 1^{e}. Here (still assuming the letters to be seven inches long) the illusion is present up to a distance of about 15 inches only. Beyond this distance a change of the nature of *confluent union* takes place, a core of blackness, as it were, running through each series of units and causing a definite visual union, so that the letters are now erect and rectangular although of distinctly ratchet-like outline.

The same descriptions may be applied to Series II (the black background series). In Series III (the grey background series) the illusion diminishes in sharpness and brightness up to 1^{3c}. In 1^{3a} sharpness and brightness are still present in the letters directly regarded. In 1^{3b} sharpness is lost. In 1^{3c} both sharpness and brightness are lost. In 1^{3a} although *geometrically* the figure consists of black and white adjacent areas with triangular ends lying on a grey background, *visually* it consists of the 'twisted cord' lying on a fragment of chequer-work background.

In Fig. 1^{3b} the brightness is due to each unit being still largely enclosed between two contrasting units.

If Fig. 1^{a} and a corresponding figure on a black background be compared with Fig. 1, it will be found that the letters in Fig. 1 (which contains equal areas of black and white) possess a sharp clearness of outline absent in the two other figures (in which there is a large excess of either black or white).

A similar triple series of variants of Fig. 2 may be obtained by the method described above, and will support the explanation offered as to the relation of the sinuosity in Fig. 2 to the resolution into units in Fig. 1.

IV. *Two coloured variants of Fig.* 1 (*not illustrated*).

Coloured variant A. In Fig. 1—

If for black squares be substituted squares of saturated red,
„ grey „ „ „ „ „ light red,
„ white simple units „ units „ greenish-blue,

a figure is obtained which presents the illusion in a diminished degree. At a distance of about 15 inches and beyond, when viewed through a transparent red medium corresponding to the red in the figure, and sufficiently intense to almost obliterate the squares, the illusion disappears. The figure, under these conditions, in fact, corresponds to Fig. 1^e. Confluent union of the now almost equally dark units on a now almost uniformly light background has taken place. Viewed, at a distance, through a transparent medium of corresponding greenish-blue the illusion is markedly increased; the figure now corresponding to Fig. 1. Increase of the illusion also takes place if the figure be viewed at any distance with very feeble illumination when the eyes have become dark-adapted. This is explained by the fact that the retinal response to stimulation by colours of very low luminosity (attributed to the retinal rods) is much less for red than for the other colours. The reds and greenish-blues become therefore relatively darker and brighter. There is now increased contrast.

Coloured variant B. If in Fig. 1—

For black squares be substituted squares of saturated greenish-blue,
„ grey „ „ „ „ light „
„ white simple units „ units „ red,

a figure is obtained in which the illusion is increased by being viewed at any distance through a red medium. This is due to the now increased contrast of the dark and light units and the dark and light squares. The illusion is abolished by being viewed at about 15 inches and beyond through a greenish-blue medium. It is also abolished by dark-adaption inspection at the same distance. In the two latter cases the abolition of the illusion is due to confluent union of the now almost equally dark units on a now almost uniformly light background.

When the coloured variant *A* is illuminated by a flash lasting $\frac{1}{8}$—$\frac{1}{2}$ second, the direct image presents the illusion in a degree corresponding to that due to ordinary inspection and is followed by a brief after-sensation by which the illusion is perceived exaggerated.

When the coloured variant *B* is similarly illuminated at a distance of three feet and beyond, the direct image presents the illusion in a degree corresponding to that due to ordinary inspection, and is followed by a brief after-sensation by which the illusion is abolished, the letters appearing black and erect.

In fact, the after-sensations of such brief illuminations of the coloured variants *A* and *B*, correspond exactly to the appearances of these figures when seen in very feeble illumination by the dark-adapted eyes[1].

V. *Construction of the circular figures.*

In the construction of the circular figures a circular area is divided by radial lines into a certain number of equal sectors (*e.g.* 32 in Fig. 3, 48 in Fig. 5). A series of concentric circles are then described at such intervals that the quadrangular divisions obtained have each three equal sides, the outer circumferential side and the two radial sides, and one side (the inner circumferential) which is slightly shorter. These quadrangular areas are thus approximate squares and on being bisected diagonally in both directions give a new group of approximate squares which is used as the basis of the background of the illusory figure to be produced. The two sets of diagonals of the second group of approximate squares are the original radial and circular lines, and on these lines rest the 'units of direction.' The 'units of direction' all bear the same relation in size to the approximate squares on which they lie, so that in the completed figure the members of each radial series of similar geometrical parts diminish in size centripetally by geometrical progression, and the members of any circular series are of equal size.

---

[1] When these observations in connexion with brief illumination of the coloured variants were made they immediately recalled to the writer a puzzling colour experiment of three years before. A screen photograph 'half tone' print ($\frac{1}{6}$ inch square) of a man's face was enlarged so that the black and white areas when of equal size were $\frac{1}{6}$ inch square. An exact copy of this was made, substituting red for black and greenish-blue for white, the colours being saturated and apparently of equal luminosity. (This figure, potentially both a positive and negative portrait, was used for some observations in connexion with retinal rivalry, a positive portrait being produced for one eye and a negative portrait for the other by the interposition of corresponding red and green-blue media.) The writer was astonished to find that the after-sensation of a brief illumination (the duration of which was not then timed), the figure being placed at 1 to 2 feet from the eyes, was usually a more or less definite positive portrait. He now finds that in the after-sensations of a brief illumination ($\frac{1}{8}$—$\frac{1}{2}$ second) of a series of saturated colour areas, arranged in spectral order, the greenish-blue appears to be the most luminous and the red the least luminous. The other colour areas appear intermediate in luminosity, orange-yellow and blue-violet being of nearly equal luminosity. These luminosity relationships correspond to those perceived in very feeble illumination of the same coloured surface by the dark-adapted eyes.

The chequer-work background is composed of a double series of dark and light broad spiral bands running in counter directions, and formed by the visual union of the black and white squares with the grey squares. The units of any one circular series all correspond to the same circle in one of two ways—either the centre of area of a unit corresponds to the centre of a black or white square; or, the marginal dividing line of two 'units' corresponds at its central part to the diagonal line of a black or white square, that is, to part of the actual circular curve. In Figs. 4 to 7 the curves are arranged in pairs; there being a distance equal to the sum of the radial diagonals of two grey squares between the pairs. The curves in each pair have an identical distribution of the angular inclinations of the units; the units of neighbouring pairs of curves being opposed in angular inclination.

As in Fig. 1 the length of each unit is approximately equal to the sum of the diagonals of three squares, except where a change in the angular inclination takes place. Here either the outer or the inner unit is approximately equal to the sum of five diagonals, as in Fig. 5.

In Fig. 3 the angular inclination of the units is the same throughout.

In Fig. 4 the units in each pair of curves are inclined in an opposite direction to that of the units of a neighbouring pair.

In each curve of Figs. 5 and 9 there are four equidistant squares on which the centre part of the units lie side by side so that their marginal dividing line coincides with the diagonal line of a square, that is, with the circular curve. Starting from the angular points of each of these four squares the units are inclined in opposite directions.

In Fig. 5 the distribution of the angular inclinations of the units is such that the apparent long diameter of each pair of curves is at right angles to the apparent long diameter of a neighbouring pair.

In Fig. 8 on each curve there are eight equidistant squares starting from the angles of which the units are inclined in opposite directions.

At the four angular points of the 'squared' circles the central point of the marginal dividing line of the units is very slightly outside the circular line.

In Fig. 9, where there is no arrangement of the curves in pairs, the distribution of the angular inclinations of the units is such that the apparent long diameter of each curve forms an angle of 45° with the apparent long diameter of each neighbouring curve.

On ordinary visual examination of this figure, with the exception of the outer curve, it is extremely difficult to isolate individual curves. Neither the actual nor the apparent nature of the curves can be definitely perceived. The general impression is that of an entanglement of curves, or of a single very irregular curve. On steady fixation, however, of some point in the figure, preferably the central point, the curves tend to isolate themselves in a varying sequence, illusory elliptiform curves, whose long diameters have apparently different angular inclinations, succeeding one another in consciousness, the four angular directions being easily discriminated.

In Fig. 6 the two middle pairs of curves correspond to the curves in Fig. 5. The outer and inner pairs of curves are of the 'untwisted strands' type. There is in them no illusion as to the geometrical nature of the curves as a whole; but sinuosity or waviness is obtained under the conditions described for Fig. 2.

In Fig. 7, starting from each end of the horizontal diameter of each curve, the angular inclination of the units is reversed.

In Fig. 10, the background of which is similar to that of the other figures of a circular type, an illusion is produced as to the direction of the actually radial 'twisted cords.' There is an opposite angular inclination of the units in neighbouring 'twisted cords,' and a corresponding apparent inclination of each cord.

The 32 radii, instead of appearing to reach the centre of the figure, appear to meet, in pairs, on 16 equidistant points on the circumference of a circle described at some indefinite distance from that centre.

An elliptical figure.

In Fig. 11 a concentric series of (geometrically) similar ellipses is treated in the same fashion as the circular curves. The short diameters are to the long diameters as 7 to 8. Starting from the vertical angles of the squares whose horizontal diagonals correspond to the long diameters of the ellipses, the angular inclination of the units is outwards from the elliptical curves. The marginal dividing lines of the central parts of the units which correspond to the ends of the long and short diameters of the ellipses coincide with the elliptical curves. The result of this treatment of the elliptical curves is that from without inwards the curves become apparently more and more circular in character; the inner curves simulating ellipses having long diameters at right angles to the long diameters of the outer curves. If the figure be rotated through half a right angle in the plane of the paper, the apparent gradations are probably better appreciated. Viewing the figure with any angle of rotation the *identification* of the curve which appears to be the normal circle is impossible.

VI. *General Properties of the circular figures.*

The properties of Fig. 1 described in six short sections on pp. 309 f. belong also to the group of figures of circular character (Figs. 3 to 11), and may be considered in the same order, noting a few new characters due to the geometrical nature of the figures.

(1) *Changes in character of the illusion, due to changes in the size of the image.* Examining each figure from without inwards, the tendency to resolution into the overlapping black and white units increases with the decrease in size of the parts. The inner curves, therefore, or the inner parts of the radial bands (Fig. 10) appear to be more broken up than the outer curves or than the outer parts of the radial bands. This is most apparent where the curves are separated into pairs, which contrast by their opposite angular inclinations with neighbouring pairs (Figs. 4, 5, 6, 7).

Where there is parallelism in the inclinations of the units throughout, as in Figs. 3, 8, 11, the resolution into units brings about *a confusion of identity* in connexion with the inner curves, that is, any one of these smaller curves is easily confused with its outer or inner neighbour. This confusion of identity results in an exaggeration of the illusion, so that the central curves of these figures show an apparently greater departure from the actual geometrical form than the outer curves.

If Fig. 3 or Fig. 11 be examined at gradually increasing distances the inner curves gradually cease to be visible, the visible curves showing a greater degree of the illusion as they approach invisibility. If the centre of Fig. 3 be compared with the centre of Fig. 4, at corresponding distances, the central pair of curves of Fig. 4 appear as a double whorl of overlapping units, while the corresponding central part of Fig. 3 simulates a continuous curve.

The apparent incurvation and excurvation opposite the ends of the evidently short diameters and long diameters respectively of the illusory elliptiform curves (Figs. 5, 6) increase with the diminution in the size of the curves. This phenomenon, in fact, is correlated with the tendency to resolution into units, and, in explanation, the theory may be applied which is offered above as to the sinuosity in Fig. 2.

(2) *Effects of angular position of figure.* If illusory elliptiform curves of equal size be compared, the illusion as to the relative lengths of the apparent long and short diameters will be found to be slightly greater when the apparent long diameters are in the horizontal position.

(3) *Images of very brief duration.* In all the figures the illusion is distinctly present in images of very brief duration ($\frac{1}{8}$—$\frac{1}{2}$ second).

(4) *Effects of steady fixation.* On steady fixation the illusion persists in all the figures; there being a distinct alternation, at time intervals (1 to 2 seconds), of the curved or radial figures with the double spiral background.

In Fig. 9, as already described, the curves tend to isolate themselves in a varying sequence.

In Fig. 3 the apparently open curve becomes broken up into its constituent closed circular curves. These, at intervals, are perceived as distinctly isolated circles having a spiral character or tendency.

In the negative after image of all the figures the alternating changes described can be distinctly perceived.

(5) The figures of circular type have not been examined by the method described in Section 5 of the description of Fig. 1.

(6) *Binocular Combination.* If any one of the figures of a circular type (excluding Fig. 11) be binocularly combined with a figure of equal size and similar structure, but having opposed angular inclinations, unit for unit, the result is the perception of a concentric series of perfect circles, or a series of normal radial bands (Fig. 10); there being an ambiguous relief of the circles or radii in relation to the background,—an impression of disengagement.

If Fig. 6 be combined with the same figure turned through a right angle in the plane of the paper, the effect is that of four pairs of concentric circles, the outer and inner pairs being level with the background, the two middle pairs being disengaged from the background.

The variants, coloured and uncoloured, described for Fig. 1, may be made for the figures of circular type and show corresponding phenomena.

VII. *An illusory figure examined in relation to a corresponding non-illusory figure.*

The visual geometrical paradox present in most of the 'twisted cord' figures is only fully appreciated by a comparison of the illusory figure with a figure drawn to imitate it. In Fig. 5^a an attempt has been made to reproduce the illusory curves in Fig. 5. Each space between the two pairs of curves in Fig. 5^a narrows and broadens as the pairs of curves mutually approach or recede, so that their nearest and farthest distances from each other are in the ratio of 1 to 3. In Fig. 5 the corresponding spaces are, of course, of uniform breadth throughout, but in an ordinary examination of Fig. 5, with free undirected eye-movements, this is not perceived. If, however, the vision be slowly swept round an interspace, closely following a circular line of intermediate black and white squares, although this uniform breadth can be now appreciated, the curves are still perceived as elliptiform pairs with long diameters alternating in vertical and horizontal directions.

VIII. *The Münsterberg or chequer-board illusion as related to the 'twisted cord on chequer-work background' illusion.*

The Münsterberg illusion may be regarded as an illusion of the 'unit of direction' type.

In Figs. 12, 13, 14, a gradual geometrical transition series, extending from the one illusion to the other, is given; and in Fig. 15, are shown in numbered divisions corresponding to each member of the series:

(1) The type of each compound unit of direction.

(2) The type of its apposition to the neighbouring contrasting unit.

(In Nos. 7 to 14 of Fig. 15, the black and white squares completing the chequer-work background are, for the sake of clearness, omitted.)

In the text-books the Münsterberg illusion is usually represented with either the black units or the white units incomplete; that is, either with thin black bands separating the white squares from each other, or with thin white bands separating the black squares. Each band represents the middle portion of a unit. The illusion is distinctly increased in degree when both black bands and white bands are present, and in such an arrangement when a broad band of grey is laid alongside the squares on either side a series of units is obtained, alternating black and white, of identical shape and size, on a background of intermediate luminosity.

In Nos. 2 to 8 of Figs. 12, 13 the middle portions of the units are apposed end to end, so as to form a continuous thin band of grey separating the squares. No. 2 corresponds to the form of the illusion found in *Pseudoptics* (Milton Bradley Co., Springfield, Massachusetts). Here the thin grey band indicates the small interval separating two card-board sheets which slide against each other, edge to edge, the little dividing trench being usually so illuminated as to represent a grey which contrasts with both white and black. The illusion is present, in a slight degree, when no separation bands are present, as in No. 1, Fig. 12. It must, in this case, be assumed that the units of direction are formed by a slight amount of confluent union of pairs of opposite obliquely separated squares of the same luminosity. This confluent union is increased by a small area, disc or square, being placed in a position exactly between the two confluent squares. The unit of direction, in these circumstances, may be represented by that in No. 9 of Fig. 13, by omitting the grey areas and regarding the triangles as squares. The illusion in Figs. 12—14, on the whole, increases in degree from No. 1 to 14, that is, directly with the *increase in compactness* of the units of direction (as displayed in Fig. 15).

IX. *The nature of the 'unit of direction' illusion.*

Without attempting to offer a psychological theory as to the 'unit of direction' illusion, the writer may indicate certain negative and positive characters.

(1) There is not present in any form of the illusion any definite suggestion of perspective (derived from geometrical details), such as is present in the Zöllner illusion, and is advanced by some writers in explanation of that illusion.

(2) As the illusion is perceived as the result of $\frac{1}{8}$—$\frac{1}{2}$ second flash illumination, eye-movements are eliminated as a factor.

In the experiments described above in connexion with the coloured variants of Fig. 1, where the illusion is abolished or increased by the sudden interposition of a coloured medium, the writer fails, by introspection, to detect, as accompanying the change, any definite image or feeling of movement.

(3) In whatever ways the illusion may be related physiologically and psychologically to the Zöllner illusion, in the writer's case the two illusions are separated by one simple visual difference.

The illusion present in the Zöllner figure, and in all the variants of that figure, is abolished on steady fixation accompanied by steady peripheral attention. The 'unit of direction' illusion is distinctly modified under the same conditions but never abolished.

(4) There are possibly present as factors in the illusion two distinct visual *integrative* processes:

(*a*) One corresponding to a line joining the centres of areas of the units. This may be looked on as a form of confluent union.

(*b*) One representing collectively the trends or tendencies of the units.

The more clearly and discretely the units of direction are impressed on the median or peripheral (as distinct from the central) region of the retina, the more the second integrative process (*b*) tends to prevail. The nearer the units are to each other in serial position, the more they approach each other in luminosity (the more, in fact, the conditions favour confluent union) the more the first integrative process (*a*) tends to prevail.

In the LIFE figures, accordingly, the maximum degree of illusion is found in Fig. 1 ('twisted cord on chequered background'), and the minimum degree in Variant 1^e (1st series of variants of Fig. 1), where the black units in series touch each other at angular points. The second integrative process (*b*) in this latter figure (assuming the letters to be 7 inches long) ceases to be active when the figure is viewed at a distance beyond 15 inches.

If either of the coloured variants of Fig. 1 (*A* and *B*) be viewed at a distance of 15 inches and beyond, through a series of coloured media, in spectral order (extending between the complementary red and greenish-blue), the illusion gradually decreases in degree as the units gradually approach each other in luminosity, being abolished when the units are of equal luminosity.

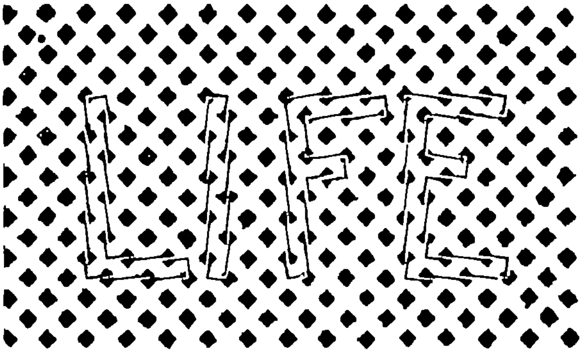


Fig. 1^{a}.

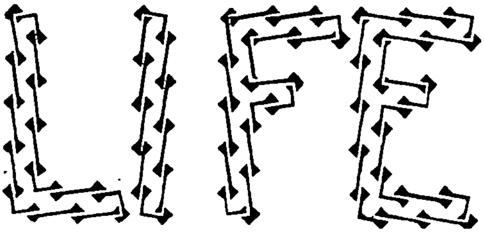


Fig. 1^{b}.

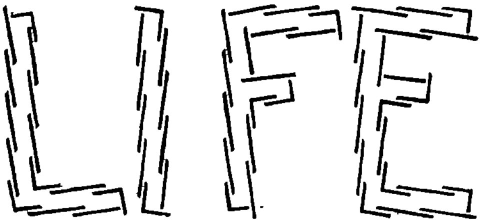


Fig. 1^{c}.

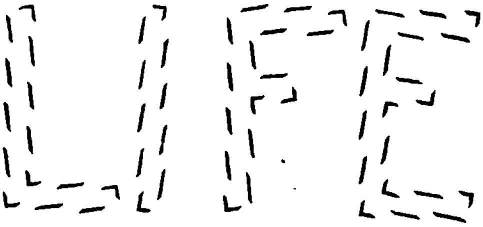


Fig. 1^{d}.

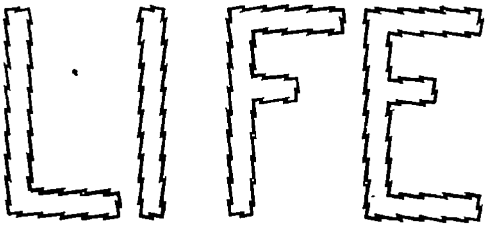


Fig. 1^{e}.

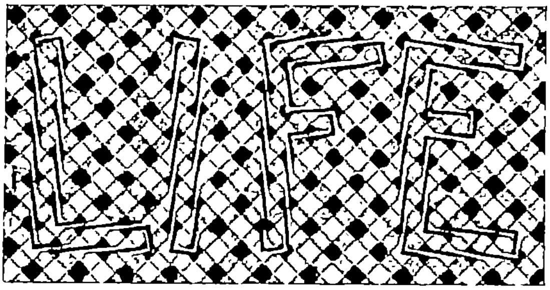


Fig. 1.

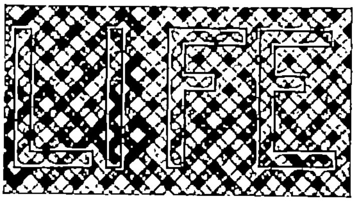


Fig. 2.

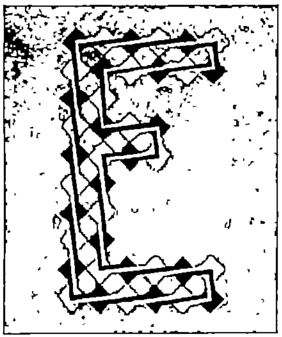


Fig. 1^{3a}.

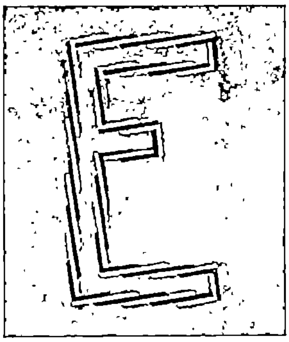


Fig. 1^{3b}.

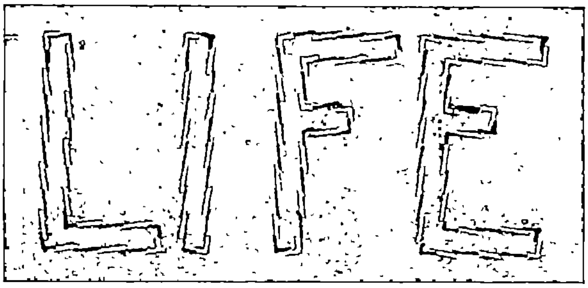


Fig. 1^{3c}.

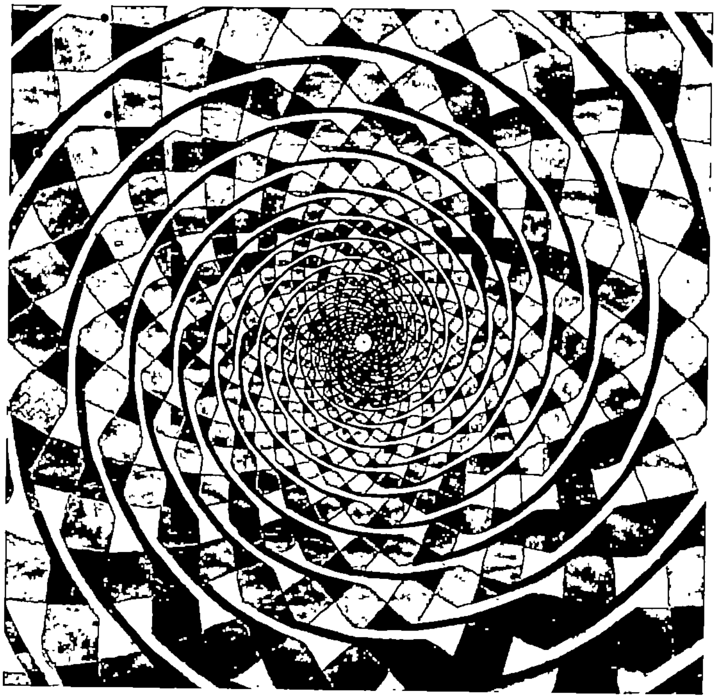


Fig. 3.

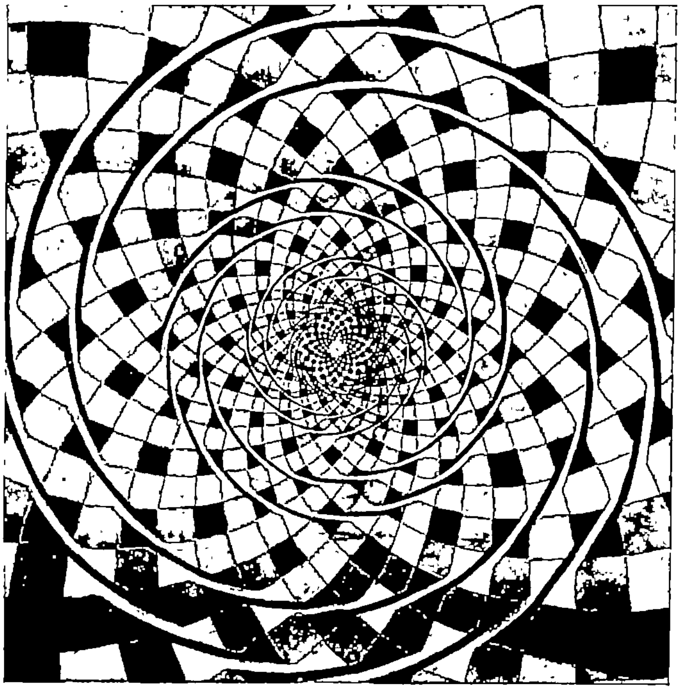


Fig. 4.

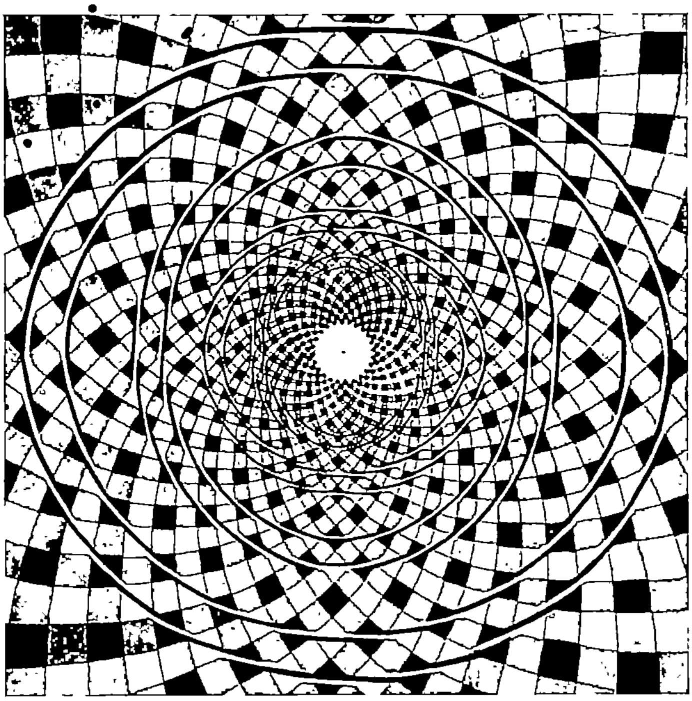


Fig. 5.

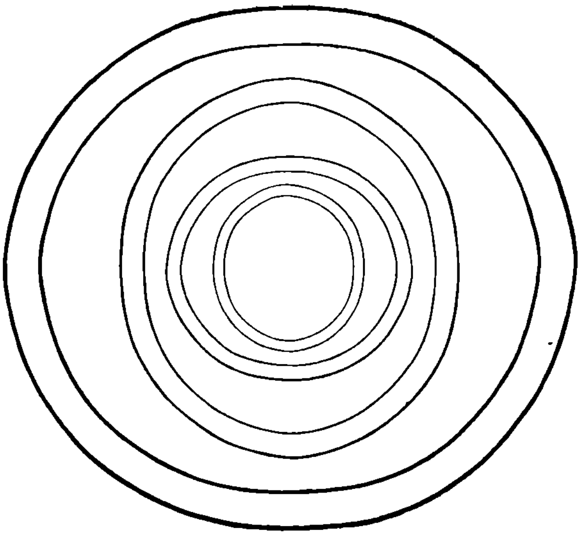


Fig. 5a.

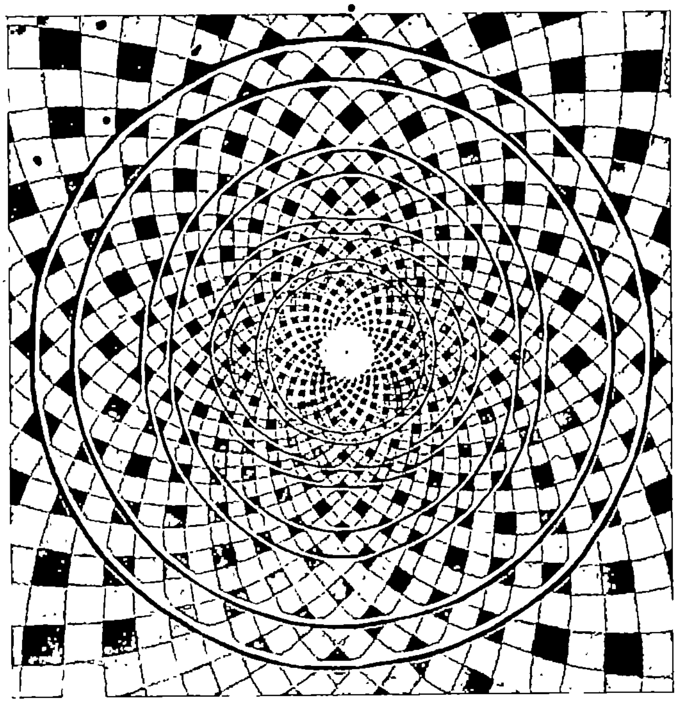


Fig. 6.

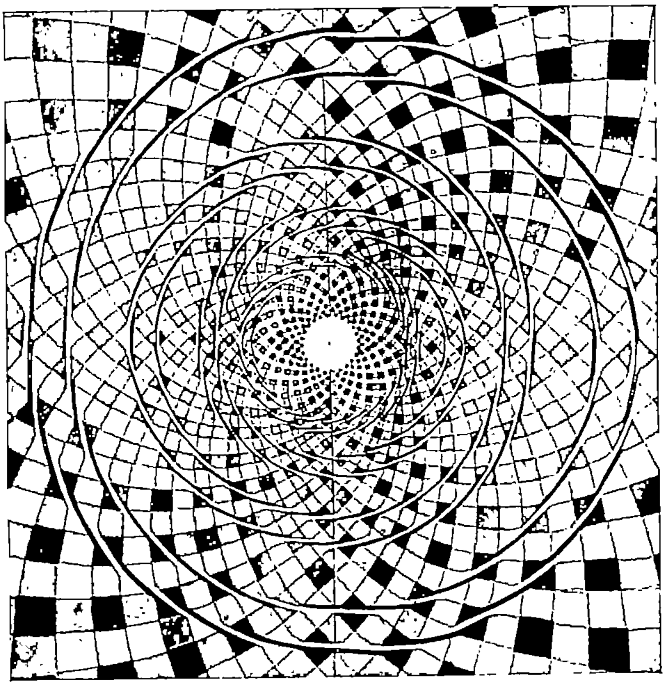


Fig. 7.

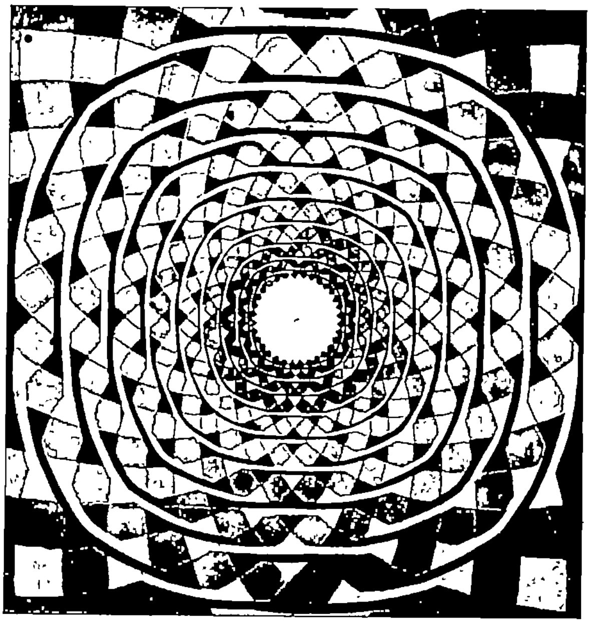


Fig. 8.

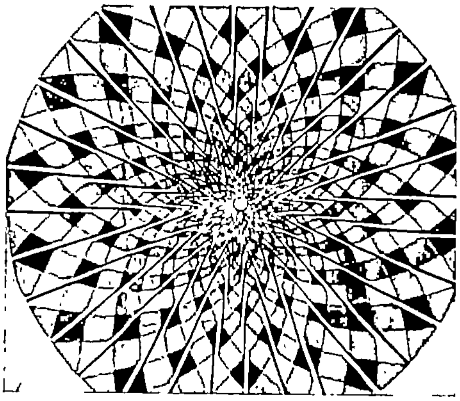


Fig. 10.

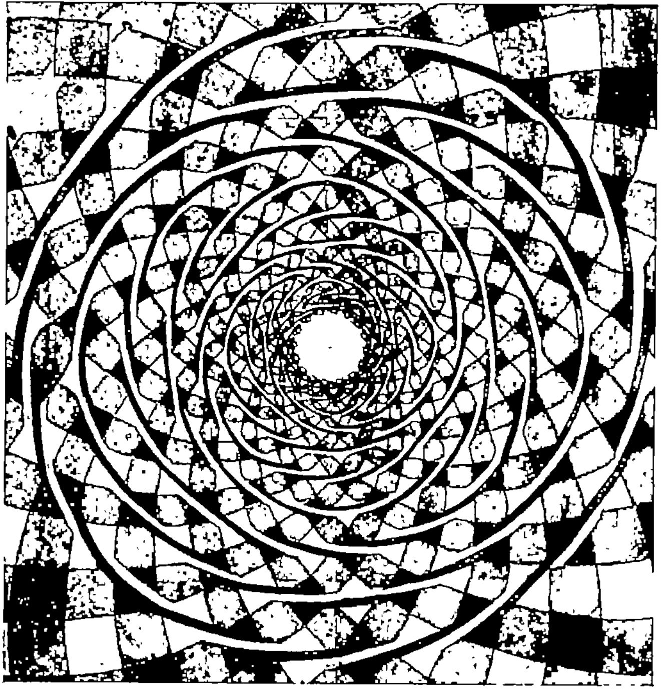


Fig. 9.

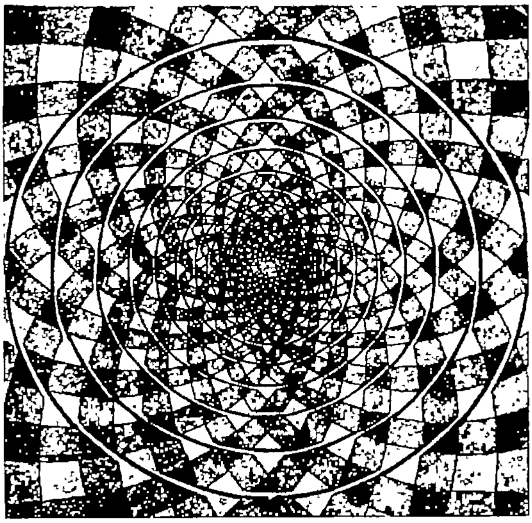


Fig. 11.

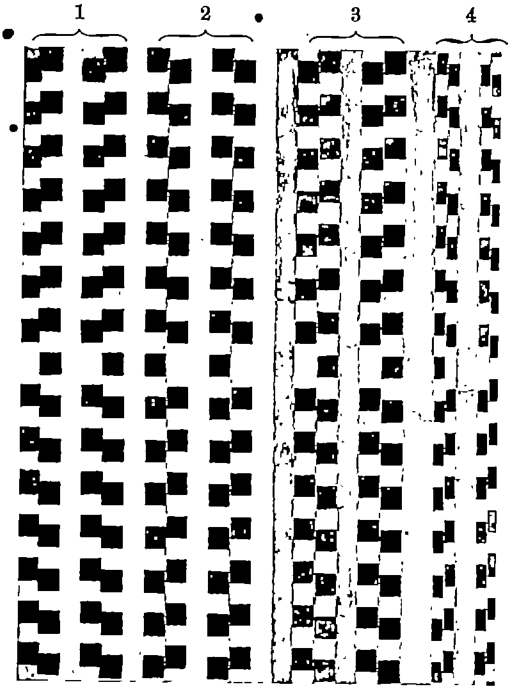


Fig. 12.

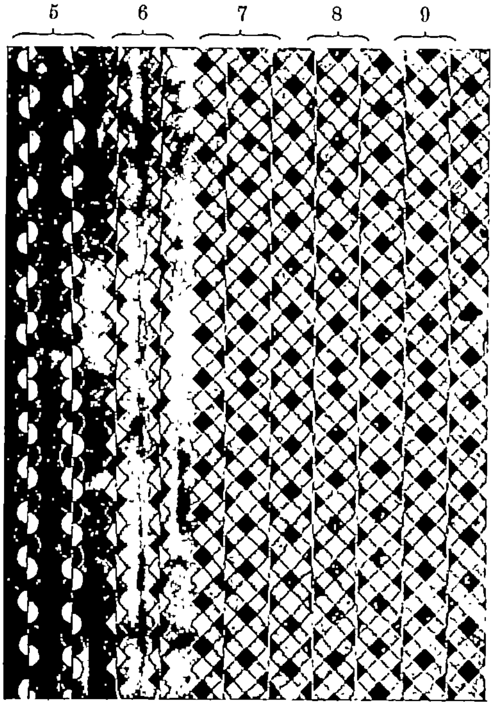


Fig. 13.

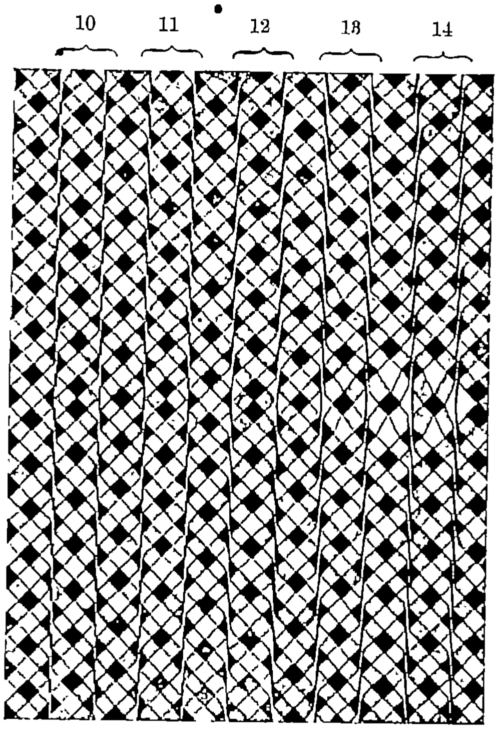


Fig. 14.

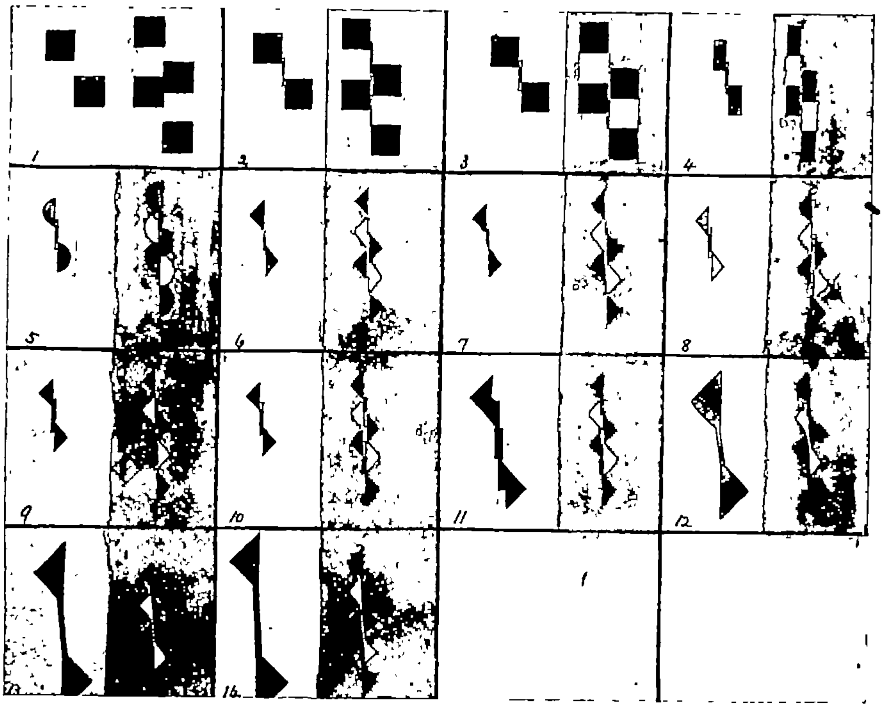


Fig. 15.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

June 15, 1907.	A suggested Physiological Basis for the distinction between a Sensation and its Revived Image, by F. Gotch. Ribot's Theory of the Passions, by A. F. Shand.
November 9, 1907.	Remarks suggested by Professor Gotch's paper on the Physiological basis for the distinction between a Sensation and its Revived Image, by Professor Carveth Read. Some points of Psychological interest suggested by a case of Experimental Nerve-division in Man, by Dr W. H. R. Rivers and Dr Henry Head.

VOLUME II OCTOBER, 1908 PART 4

ON THE DIFFERENCE BETWEEN PERCEPTS AND IMAGES.

BY CARVETH READ.

1. *Prof. Gotch's paper, upon which this one is founded.*
2. *Perception, as contrasted with images, is a state of relative restriction of consciousness.*
3. *This difference is due to the reflex of perception that adjusts the sense organ.*
4. *Hence we may explain the aggressiveness, outness, steadiness, etc., of perception.*
5. *The principle illustrated by dreams and hallucinations;*
6. *and also by attentive thought, criminal perversity, etc.*

1. THE difference between Percepts and Images, which seems so obvious, and for practical guidance is in fact known to everybody, has not yet been determined to the satisfaction of psychologists. Several distinguishing features of the Image are pointed out in text-books; yet fresh attempts to make the difference clearer are frequently published; and the reason of this sense of something being still wanted may be, that the differences hitherto recognised are merely descriptive and that the ground of them all remains to be shown. It is, perhaps, too much to hope that I shall succeed in supplying this desideratum.

In the early summer of 1907 the nature of hypnosis, plural personality and criminal perversity had been puzzling me a good deal. Certain resemblances between these mental states seemed to indicate some common ground of them all, and explanatory ideas sometimes presented themselves in vague outlines only to be rejected again. It occurred to me that they must be generally referable to abnormal or pathological states of attention, and that they must be connected with hallucination; but it did not cross my mind to consider them in relation to normal perception: such is the power of opposite terms, like "normal" and "abnormal" to hinder one in thinking. Then in

June (the 15th), at Oxford, Professor Gotch read before the British Psychological Society his paper *On a suggested Physiological Basis for the distinction between a Sensation and its revived Image*; and that immediately suggested to me a generalisation which I now venture to lay before the Society[1].

Professor Gotch began by observing that "the conscious distinction between any sensation and its revived image must rest upon some fundamental difference in the physiological processes aroused in the two cases"; that this difference could not be related to the origin of nervous impulses giving rise to sensation in specialised sensory organs, nor could it be connected with the mere intensity of the effect. Hence it had generally been assumed by physiologists that wider cortical areas were concerned in the case of images; but no explanation had been given of how this wider area of disturbance could furnish recognition marks for the significant consciousness of revived images. He then offered the following suggestions bearing upon this point:

"In any *sensation*, however aroused, the total sum of the physiological changes comprises (1) that central excitatory activity which may be termed the specific sensation basis, (2) the simultaneous occurrence of excitatory central activities of other sensory types, (3) the residues of all the activities of afferent type which were the more immediate predecessors of (1).

"In every *revived image* the total sum would include all these, but in addition there must be (4) the development of those excitatory states which formed the more immediate successors of (1).

"Hence in a *sensation* the whole central activity ends abruptly as regards time with that phase of the whole physiological sum indicated under (1); whilst in a *revived image*, the total sum of the physiological states, even if precisely the same processes (1), (2), and (3) recur, is modified by the presence of (4) and the absence of the abrupt termination."

In the former case, as a result of the abrupt termination of the physiological process, sensation probably attains "a degree of vivid intensity greater than it could have if the setting were more extended in time": whereas in the latter case, the presence of (4), excitatory states forming the more immediate successors of (1),—which can only be present in revived states, and thus constitutes the physiological index of this revival,—is "a recognition mark which, in consciousness, is interpreted as a revived image[1]."

---

[1] The substance of this paper was read before the British Psychological Society on the 9th of November, 1907. Some alterations have been made at the suggestion of Mr McDougall and Dr Mott.

2. These views were to me extremely significant; what specially interested me was the abrupt termination described as characterising perceptions as such. Taking the numbered processes in the above passage, we may call (1) primary cortical excitement, (2) perceptual irradiation, (3) residuary influences, and (4) associations. For the purpose of the present paper (3) may be neglected. Together, (1) and (2) constitute the nervous basis of a percept or perceptive system, an immediate sensation with its associated sensory elements. Supposing the percept to be something seen, we may symbolise it by *Vtk*, where *V* stands for immediate visual sensations (1), and *tk* for the tactile and kinaesthetic complex, corresponding with the perceptual irradiation (2).

There is some difficulty in naming the secondary elements of a perceptual complex that are excited by irradiation from the primary sensory impression. Although they give the most important qualities of the thing perceived, they are not themselves directly presented or represented: they are only distinguishable by reflection. Yet they cannot be considered as merely modifying the primary sensation elements, for they belong to different kinds of sensation, with entirely different physiological correlatives. On the other hand, if we follow the physiological indications alone we may be misled, as I think Professor A. Binet is, in his *Psychologie du Raisonnement* (c. L.), when he defines perception as the mental process that completes a sensation by an escort of images. To call the secondary elements of perception "images" is to raise an exaggerated idea of their independence or distinguishability in perceptual consciousness. And this may easily happen if we say:—"What is a sensation? Consciousness due to peripheral stimulation. What is an image? Consciousness due to central re-excitement of cortical sensory areas. But of such kinds are the primary and secondary elements of perception; therefore a percept is a sensation plus images." We need some terms that will neither break up the unity of the percept into such separate elements as sensations and images, nor yet obscure the fact that perception is a synthesis of different kinds of sensory elements. I propose, as the best way of meeting this difficulty, to call the secondary elements (tactile and kinaesthetic in things seen) "subpresentations" or "subrepresentations." "Subpresentation" seems the better expression if we consider the immediacy with which all the qualities of an object seem to be given in visual perception; but "subrepresentation" has the advantage when we consider that the secondary elements are centrally excited, or when we attend to the nature of auditory or olfactory instead of visual percepts, and observe that in their case the secondary elements are distinctly represented. Perception is described by Spencer as "presentative-representative"; and this double nature of the process should not be pushed so far into the background as to be lost sight of altogether. But I am digressing.

---

[1] The quotations are made from the syllabus of Professor Gotch's paper, that was circulated to members of the B.Ps.S. Compare the views of Prof. William James (*Principles of Psychology*, chapter XIX (on Hallucinations)).

The essential point of Professor Gotch's paper, as it appealed to me, was that perception as such ends abruptly: (1), the immediate sensation, with (2), the subrepresentations simultaneously occurring, constitutes a total which is in a manner punctuated; does not in natural course excite (4), associations; but, compared with images, is a state of relative dissociation, a restriction of consciousness. Some presumption of the truth of this may be raised in anyone's mind who will let his eye glance round the landscape just fast enough to note the several objects that compose it. Perception, he will observe, passes from object to object by a sort of check-action; each object, if one stops there, is final, and may be fixed for some time, remaining steady without effort. But with images it is otherwise: either they continually undergo transformation into other images, or they fade away. Seeing that images sometimes fade away, it must be admitted that they do not always differ from percepts in giving rise to associations. In such cases we may suppose that the correlative neural energy is dissipated by a diffused leakage, which in no direction produces a sufficient disturbance to give rise to further images; just as the energy of a moving body may be entirely dissipated in producing heat. It must also be acknowledged that the rapidity with which a multitude of miscellaneous objects can be observed and recalled may be greatly increased by practice: there is the well-known case of two conjurors, father and son,—the Houdins. But we may suppose that, in such a case, the several objects remembered are not *severally* observed, but rather that they are registered in surveying them, and that the register is afterwards read off.

The occurrence of (4), then, after an image, is a fact of association, and its non-occurrence after a perception constitutes a dissociation; and this difference I shall endeavour to trace to a particular cause.

It is a dissociation because we have no strictly new percepts; and therefore with every percept in so far as it is a reinstatement, total or partial, of former ones, there is a tendency to revive the successors of those former ones. There is no such revival as long as it remains a focal percept; and so far it is dissociated.

But, to avoid misunderstanding, I will point out three ways in which a perception may excite associations. In the first place, in the human mind, quite normally, every perception is in some sort "understood," or classified, usually by means of a more or less general name, e.g. as a "blackbird," a "bird," a "moving object." But this is merely the excitement of a more comprehensive system in the same way as perceptual irradiation is excited: *V*, besides exciting *k* and *t*, also excites *N* (some name) and, through *N* subexcites everything that *N* denotes, and gives a certain predominance in the percept to that which *N* connotes. In perception the union of sensation with understanding is characteristic of human cognition. Spencer observes (*Princ. of Psych.* § 311) that the name may be considered as an additional attribute; and so it may; but it is something more, because the subexcitation of its denotation and the emphasis on its connotation take up the percept into a larger system, and undoubtedly facilitate the revival of further associations. Yet the larger system is itself relatively dissociated; it does not lead of course into other systems. If you perceive (and thereby conceive) a blackbird, perched in a hawthorn, behind which runs an ivied wall with elm trees overhanging it, "blackbird," "hawthorn," "wall" and "tree" are in a manner punctuated; each has a certain finality and self-sustaining power.

In the second place, a percept may excite any ideas that stand in a prepared connexion with it; as when on observing a knot in your handkerchief, you are reminded to write a letter; or as when a thief plunges down an alley at the sight of a constable; or as when Newton sees an apple fall and thinks of the moon (never mind about the truth of this story). The first of these cases is a prepared association, and in some measure so is the second; but the second partly and the third altogether depend upon a principle which Mr McDougall has suggested (*Brain*, Vol. XXIV. p. 423), namely, the determination of revival by the charge of free energy in distal neurones; for the thief's ideas of arrest and imprisonment and Newton's thought of the moon as a falling body are correlated with groups of neurones which previous interest has brought to a high state of nutrition and instability; so that any leakage in their direction immediately throws them into activity. And here we may see the importance of names or concepts in directing the course of revival. The thief plunges down an alley when he sees something not merely as a 'man,' but particularly as a 'constable' or (strictly) as a 'Bobby': Newton thinks of the moon when he sees something not merely as an 'apple' but generally as a 'falling body.'

In the third place, if in the presence of any percept we take the attitude of expecting associations to be revived, they readily occur to us; but then the present percept is no longer fully in focus. As Ribot says, quoting with approval from Sully, the attitude of expectant attention "involves a continual transition of mind from one aspect to another, one set of suggestions to another. Hence it would be more correctly described as making the object the *centre* of attention, the point from which it sets out and to which it continually reverts." I shall presently return to this case.

3. Dissociation, restriction of consciousness, is usually referred to as explaining dreams and abnormal or pathological conditions, such as hypnosis, manias, criminal perversity, plural personality; but in fact it is normal in attention. There is no attention without dissociation; and plainly every focalised perception involves attention. In both reflective and perceptive attention restriction of consciousness is necessary to sanity; it is another name for concentration, without which nothing could be steadily determined 'either as Being or as Non-being.' We ought therefore to inquire into the conditions of such restriction of consciousness and into the difference of the conditions in normal and in abnormal restriction.

In lecturing upon percepts and images I have long been accustomed to point out a distinction between them which seems not to be in the books, though some of them give a long list of other distinctions; it is this: in perception the object holds the eye, but in phantasy the mind's eye has to hold the image. This difference seems to be due to the definite reflex of perception which adjusts the sense-organ and maintains the continuity of the stimulus. Thus a circular activity is set up: the stimulus excites an adjustment, which maintains the stimulus, which maintains the adjustment, and so on. An image may also be assumed to excite a reflex adjustment, but not by an arc of the lower level resulting in a definite adjustment of some organ to external conditions; and of course there is no circular activity sustained by stimulus at the periphery. Therefore the image has to be sustained by an arbitrary generalised adjustment, such as we are conscious of in reflection, which involves a reinforcement of the reflex from some source of interest. *Some* kind of circular activity seems to me to be necessary in order to maintain any sort of attention, normal or abnormal; but it may be produced not only by the adjustment of a sense-organ, but by any active tendency of the organism that is spontaneously interesting.

There is also, I presume, a general efferent discharge accompanying both percept and image, affecting muscular tonicity and glandular functions; but this, being no more directed in one case than in the other, is merely weaker for the image; the difference is merely in the amount of disturbance produced; and it is generally admitted that the difference between percept and image is not merely one of amount or intensity.

To explain, then, how it is that the reflex adjustment of the sense-organ produces the dissociation that is characteristic of perception, I turn to a consideration that Mr McDougall has often urged, namely, that the discharge of any neurone or system of neurones in one direction inhibits the possible discharges that might take place in other directions; drains the system, in fact. Thus, in the case of perception, the efferent discharge that adjusts the sense-organ inhibits the tendency to cortical irradiation into associated systems. There is, therefore, dissociation: and for the reason that the nervous energy excited by the stimulus cannot escape freely by cortical paths, if disturbance increases within a limited area it produces the intensity that is the most noticeable characteristic of sensation; provided that the stimulus excites a greater disturbance than the reflex of adjustment is able to relieve; whilst with faint stimuli there are faint sensations, but the reflex of perceptual adjustment still gives the sense of outness and a certain finality to the experience.

A subordinate consideration is that, as we have seen, every percept must have in latency numerous divergent associations, whose competition at the threshold of consciousness (if you will allow the figure) weakens each of them, and facilitates their inhibition by efferent drainage.

Normally, indeed, perceptual dissociation is only relatively complete, partly through the influence of concepts, and generally through the oscillation of the psychophysical process which is specially active in the attitude of expectation. If you glance at the wall, it holds the eye; but if, taking the expectant attitude, you ask yourself—"what does the wall-paper remind me of?"—then, supposing the paper to be a plain dull green, you may think of foliage, or grass, or a billiard-table, or the paper of a friend's study, or the picture that formerly hung in the spot you are watching. Now it may be observed that, during this process, the wall ceases to be focal and the adjustment of the eye relaxes; the wall is, as it were, veiled or obscured by images or thoughts. In fact attention has shifted to the ideational level, involving the arbitrary or generalised adjustment characteristic of reflection, which relaxes the perceptual reflex and at the same time maintains the images.

Efferent drainage in adjusting the sense-organ gives reality to perceptions and inhibits ideas; and the drainage of reflective adjustment gives a relative prominence to ideas by obscuring perceptions. But voluntary attention normally raises no image to an hallucination, though Goethe and others are said to have come near to it; and the reason of this is that its voluntary character involves associated ideas, and therefore cortical drainage. Still, when we try to raise an image to the value of a percept—if, for example, we image persistently a red cross to see whether it will produce a "negative image"—our chief effort is to exclude the play of association, that is to say, to inhibit any cortical discharge; we endeavour to dam up the energy that is generated by central excitement, until it rises to the power of peripheral stimulation. The involuntary rapture of poets is a state of profound dissociation in which the world of common experience is shut out, ideas become actualities, and conduct may be influenced in such a way as to excuse those observers who have confounded this condition with insanity. It is the direct manifestation of an instinctive activity, narrower and better organised than anything of the voluntary kind.

4. The distinctions usually pointed out between percept and image may be derived from the fact that perception involves the adjustment of a sense-organ. First, the percept is described as having "aggressiveness" (Stout, *Manual*, IV. 1, 4). It catches the eye or ear; the reflex holds the organ and the connected cortical field. Upon this ground, I take it, however obscurely recognised, the Stoics, Hume and Kant treated sense-perception as the test of reality. In perception the mind is under the influence of a power not itself, and so belief exists.

Secondly, whilst it is true with certain qualifications that "sensation-intensity" depends on the energy of the stimulus, the power of the reflex-adjustment of the organ concerned, on the other hand, depends upon the interest of the stimulus, whether biological (as in the case of a slight movement), or from curiosity, or on aesthetic grounds. If any experience becomes painful, other reflexes are excited. Pain has the intense and restricted quality of sensation; it excites strong reactions, and prevents one from thinking; whilst if one can think the pain is obscured. Similarly with violent pleasure. But the difference between percept and image is not due merely to the superior energy of an afferent over a central current. In fact the intensity of sensation is not the important thing; for (as it seems to me) some sensations may be fainter than some images; though, certainly, the comparison is difficult to carry out. Objectivity, outness, compulsiveness is the important matter. It is well known that, without any pathological conditions, an experience may be mistaken for a perception even when there is no stimulus. One example of this occurs when the audience at a concert seems still to hear a *pianissimo* passage prolonged, although the instrumentalist has ceased to play and only pretends to be playing; another when we are anticipating a signal and, seeming to hear it, react, although it has not yet been given. In both cases the explanation is the same, namely, that strained adjustment of the sense-organs has given the character of sensation to a product of central excitation.

Thirdly, that a percept is steadier than an image is obviously due to the adjustment of the sense-organ. And similarly with all the other distinctions: the image is less definitely localised and related than the percept, less distinct, complete and detailed; because (in the case of vision, where this distinction is plainest) adjustment brings the thing perceived upon the macula; and surrounding objects affecting the margin, even if the eye remains motionless, are interpreted according to the custom of vision; moreover they all excite reflexes and contribute to the total tension of adjustment; but for the image there is no such framing or control. Again, an image is less dependent than a percept on our movements, for with every movement there must be a readjustment of the perceptual apparatus to meet its new relation to objects. On the other hand images are the more dependent on voluntary control apart from our movements; for we immediately think of whatever thing may interest us.

5. That perceptions depend somehow upon dissociation is confirmed by the phenomena of dreams: assuming that physiological dissociation, or discoordination, of the cortex is one condition of sleep, it explains (1) the sensation-intensity of dreams as due to the hindrance offered by dissociation to the escape of disturbances set up by sensory or organic stimuli; and (2) the exaggeration or misinterpretation of those stimuli

as resulting from hindrances to comparison and criticism. On the other hand, the difference of dreams from true perceptions, the incoherent flux, continually dissolving and changing, that is a common mark of dreams, may be explained by the absence of any definite adjustment of the sense-organs. When dreams have a certain coherence and rationality, we may put it down to the strict organisation of the psychophysical systems that are excited; so that perhaps Plato is justified in declaring that the dreams of the wise are more rational than the dreams of the rest of us. Some of those observers who report that in remembering and thinking they have no images, add "except when falling asleep," that is, when the brain is becoming physiologically dissociated. There is a good deal of evidence that this state is favourable to hallucination (Parish, *Hallucinations and Illusions*, c. IX.); and in such minds an image thus arising may be considered as a stage toward hallucination or dream. That they have images at all shows that their thinking is not abnormal, but stands at one end of a series at the other end of which stands picture-thinking, whilst between these are many grades of fragmentary and schematic symbolism. In fact the whole series may be represented in the experience of one mind: passages of distinct visualisation may be interspaced with faint sketches and even with movements of thought quite effective yet presenting no determinate images. Such, at least, is my own case. The absence of imagery cannot be put down to total absence of synaptic resistance; for this would imply either complete dissolution of consciousness, or such advanced organisation as to exclude originality. May there not be a rapid and unnoticed escape of energy by way of Broca's convolution?

Hallucinations are closely allied to dreams, and may be explained as a failure of association or a restriction of consciousness: an image rises into a percept when the liberated energy which it presupposes cannot escape by cortical paths. Probably it is excited in some indirect way by a present stimulus (Parish, *op. cit.*, c. IV.); but the indirectness, which amounts to central excitation, distinguishes it from an illusion of the occasional or abnormal type. In such an illusion the false perception results directly from a present stimulus and comprises the sensation normally produced by that stimulus, as when a surplice hanging on a peg is mistaken for a ghost. In hallucination the false perception does not include the sensation normally produced by the present stimulus: the stimulus may be organic and the hallucination objective; as when some disease of eye or ear makes one see a ghost or hear threatening voices. In such cases the reflex of the present stimulus may have no effect upon the hallucination; but since it is objectified, so that I see a ghost in a certain place, or hear a voice from a certain distance and direction, an adjustment of the sense-organ is implied. Our hypothesis may require that any image that does not fade away nor yet revive associations, should tend to become an hallucination, and that if it reaches that state it should excite a sensory adjustment. That is, finding no relief by general leakage or specific cortical discharge, the energy of the correlative neural process escapes by the motor path. Let me put it in this way: in perception and hallucination two conditions are the same: (1) dissociation, and (2) adjustment of a sense-organ; in perception the adjustment causes dissociation; in hallucination, dissociation gives occasion to adjustment.

Suppose it to be generally true that a perceptual system of neurones discharges freely in only one direction at a time. If any percept *Vtk* is aroused by a peripheral stimulus, it starts an excito-motor and a sensori-motor reflex adjusting the sense-organ, and if interesting a further voluntary adjustment. Contraction of the muscles of adjustment opens the kinaesthetic loop and establishes a circular activity. Association paths (other than those concerned with naming) are closed by the efferent drainage. Thus we have sensation, adjustment, dissociation and the circular activity that signifies external control and therefore reality.

In hallucination the system *Vtk* is indirectly excited through some association path, and (under unknown pathological conditions) all other association paths are closed. The disturbance, therefore, can only escape by producing an adjustment of the sense-organ; which opens the kinaesthetic loop and establishes a circular activity. So far there is a close resemblance between the physiological conditions of hallucination and perception. But may it not be that an hallucination, being centrally initiated, does not give rise to the lowest or excito-motor reflex; and may not this be the reason why an hallucination does not always command belief?

I have observed a few pages back that perception hardly exists in the human mind without concepts, and have suggested elsewhere (*Metaphysics of Nature*, c. XII., § 1) that the length of human childhood serves the purpose of interlacing perception with the higher growths of imagination and thought; but if the above hypothesis be true, that there is an opposition between sensori-ideational and sensori-motor discharge, it may point to a certain conditional independence remaining to the perceptual plane; such, indeed, as we seem to experience when absorbed in carpentry or other narrow perceptual activities. Perception here does not excite associated ideas, because it primarily excites actions.

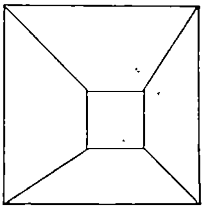
6. In *Leviathan* (c. III.) Hobbes distinguishes two sorts of trains of thought, the unguided and the regulated, by the absence or presence of some desire or design. "From desire ariseth the thought of some means we have seen produce the like of that which we aim at; and from the thought of that, the thought of means to that mean; and so continually till we come to some beginning within our own power. And because the end, by the greatness of the impression, comes often to mind, in case our thoughts begin to wander, they are quickly again reduced into the way." Attention, that is to say, depends upon the interest of some active tendency of the psychophysical organism, which persists (in effective people) until it is gratified. During the process every convergent step strengthens it, every divergent step re-excites it; by such an active tendency, therefore, a circular activity is established until the end is reached. The active tendency expresses itself in a dominant idea, and sustains it by that generalised adjustment which is characteristic of reflection. The stability of the dominant idea thus fixed limits mental discourse, in some measure, to the system of subordinate ideas. A system of ideas is like a percept in this that its internal associations are relatively open, and its external associations are relatively closed. Physiologically we may define a system as any group of neurones that normally has its internal thresholds lower than its external thresholds: that is to say, communication between all the members of the group is easier than it is between the group as a whole and other groups. The naming of any idea-system increases its separateness by lowering the internal thresholds; because the association of each element of the system with the name is a connexion additional to their direct associations with one another. In saying elsewhere (*Metaphysics of Nature*, c. XII., § 7) that the name "spreads a sort of skin" over the ideas comprised in a system, I used a misleading expression: the influence is internal, not external, but the relative effect as to other systems is the same.

In attentive thought, then, I suggest that the efferent drainage involved in the fixation of reflective adjustment is correlative with a raising of the external thresholds of the system concerned, and therefore with a relative dissociation; just as in perception the adjustment of a sense-organ has the effect of closing association paths. But as the adjustment is less definite in reflection, so, for the most part, is the dissociation less steady. A further difference between a percept and an idea-system is that, in the former, the internal thresholds are so low as to give rise to "complication"; that is to say, the subrepresentative elements are simultaneous with the leading sensation; whereas in the systems of memory, imagination and thought, the contents or subordinates of a dominant idea have to be unfolded *seriatim*. In this process, since each subordinate idea may have many other relations, we continually find ourselves led into the sphere of other systems, which may be incorporated or rejected according as they subserve, or not, our dominant idea. In voluntary attention, dissociation from irrelevant systems is less complete than in spontaneous attention, because it presupposes a more comprehensive field of regard and distracting solicitations, so that attention has to be continually renewed by recurring to the original interest, until the derivative interest becomes secondarily automatic, or "implicitly voluntary," as Professor Stout calls it. Similarly, if I try to form and maintain a definite image—say a red cross on a green ground—as there is no stimulus or circular activity, it has to be held or incessantly recalled by repeating a verbal formula (in my experience).

In criminal and moral perversion there are dominant ideas maintained by atavistic or morbid instincts, whose recurrent activity is correlative with dissociations that cut off the systems of normal restraint. In cases of plural personality, there always appears to be (if I may judge from the description of cases) a change of the coenaesthesis. This change carries with it an alteration in the subject's active tendencies or in their proportionate influence upon his character. A new dominant idea of self is then established in harmony with the new cast of feeling and the new purposes and plans. Purposes and plans are far more important to personality than mere memory is; and they restrict or modify the memory, and produce dissociation in relation to everything that does not subserve their gratification: so that even events in the organism's experience that are remembered may not be assimilated by the new system of personality if they do not agree with its own feelings and tendencies. They are referred to the other person.

Of course, I do not offer these considerations as an explanation of the phenomena of discoordination so far as they are pathological; but merely as connecting them with normal psychology. Alcohol and mania bring about changes of the coenaesthesis, of the active disposition, of the self-idea and memory; but in these cases the mischief seems to be primarily caused by cortical discoordination. In sleep and hypnosis, too, dissociation is usually connected with antecedent fatigue, or something akin to fatigue that may be produced by suggestion, shock, etc. Perhaps, however, some light may be thrown on all these things if it is true that in the normal waking state efferent drainage and central dissociation are complementary.

Dissociation is never absolute even in deep sleep; and assuming that the direction of a discharge when it occurs is the direction of least resistance, degrees of resistance may be supposed to depend upon either certain conditions at the synapses or the varying irritability of distal neurones. Consider what is implied in the interpretation of an ambiguous illusion, such as the two squares. Call this diagram *A*. It may be seen merely as a diagram; but it usually appears either as the interior of a chamber (say) *B*, or as a protruding boss (say) *C*. These two illusions alternate; and the explanation usually given is that *B* and *C* are two perceptual systems either of which may be excited by *A*; that if *B* is first active (being perhaps the more familiar), *C* is shut off; but that when *B* tires, *C* becomes active. From this we learn (1) that there are such things as perceptual systems which give definite meaning to sense-impressions; and this enables us to understand the existence of idea-systems such as I have described as represented by dominant ideas and subordinate ideas; so that as we *see* by the simpler systems, we *think* by more complex and comprehensive ones. (2) We learn that perceptual irradiation does not take all possible directions, but is strictly confined to this path or that. In the above illusion *B* and *C* are never active together, but only one or the other obtains. This, I think, indicates the possibility that a similar alternity may exist between central association and efferent drainage. (3) Supposing that the illusion *B* having been active, *C* succeeds it, because *B* is tired; this seems to me easiest to understand as the result of exhaustion and decreased excitability in the circuit of neurones constituting physiologically the system *B*, so that the direction of *C* becomes the one of least resistance; and as soon as *C* is opened *B* is closed. Now in arguing that perception is in some measure opposed to association, I have assumed that the efferent paths adjusting the sense-organ are more open than those leading to associated neurone-systems; whilst if associations are aroused, the efferent current is weakened. The effects are the same as in the alternation of illusions; though the cause of the diversion of currents may not be the same as in fatigue. Then (4) we learn from the



alternation of illusions that if nervous energy meets with resistance in one direction it escapes in another; and this agrees with the view that in hallucination, central discharge being resisted, energy escapes by adjusting the sense-organ.

It is generally admitted that the mind is composed of perceptions and ideas, which are connected not indifferently but in such a way that certain ideas are excited by certain ideas more readily than by others. Transition from one to another is easy in some cases and in other cases difficult. Ease or difficulty of transition, however, is not something absolute, but depends in many relations upon the direction of psychophysical activity. When such activity is effective for cognition or thought it always has a circular character, either the motor circuit that maintains the adjustment of a sense-organ, or the circuit involved in the progressive satisfaction of some interest as represented by a 'goal-idea.' The effect of this fixation of perception or thought is, on the one hand, to limit the range of consciousness, so that disturbing or irrelevant associations are shut out; on the other hand, to increase within that range, the steadiness, vividness, clearness and coherence of cognitions. If the limitation of consciousness is produced not by any normal circular activity but by physiological dissociation of the cortex, as in dreams, vividness results without coherence, because of the failure of comparison; though even dreams that arise from some persistent cause, such as an awkwardly placed limb, exhibit some corresponding persistence of contents. When circular activity is established it may be put an end to either by fatigue, as when the arm of a hypnotic subject in whom catalepsy has been induced slowly sinks, though he is unable to lower it voluntarily; or by the diversion of energy in consequence of the superior attraction of some new stimulus or new interest. Under fatigue we become incapable of sustained attention, and may go to sleep, or may turn irregularly from one subject to another (as in the illusion described above), or may show symptoms of delirium. The diversion of energy from one percept to another, from percepts to ideas, from ideas to percepts or to other ideas, according to the direction of interest, is common enough in normal life. Unusual difficulty in such diversion is sometimes a symptom of insanity. Too easy diversion through the failure of normal interests is another symptom of insanity, and runs out into delirium similar to that which sometimes appears in fatigue. Various types of mental pathology illustrate these principles in exaggerated forms, so far as one may judge merely by superficial symptoms.

AN INVESTIGATION OF THE COLOUR SENSE OF TWO INFANTS.

BY WILLIAM McDOUGALL, M.A., M.B.

First indications of appreciation of colour by L.—A new method of experimenting tried—Evidence of colour appreciation obtained by the method in the 21st and 22nd weeks (Table I)—The method systematically applied to L. throughout the 6th and 7th months (Table II)—The new method improved in detail and applied to A. in 8th and 9th months (Table III)—Discussion of results—Comparison of the method with that of Prof. J. M. Baldwin—Summary of principal conclusions.

I HAVE attempted to obtain evidence, in the case of two of my children, as to the age at which they began to be capable of appreciating colour and as to the order in which the primary qualities of colour-sensation were developed. Although the number of observations I have been able to make is insufficient to establish any positive and general conclusions, it seems worth while to publish a short account of these observations; for the method I adopted was, I believed, novel and is, I venture to think, superior to any other with which I am acquainted; and it may be hoped that my description will lead others to make similar experiments, for any one observer can hardly hope to obtain a mass of data sufficient for the establishment of conclusions of general validity.

Of the various methods of experimenting of which I had any knowledge at the time my observations were begun (June 1901), that of Prof. J. M. Baldwin[1] alone seemed capable of throwing any light on the state of the colour-sense of young infants, or on the early development of the colour-sense. Baldwin has sufficiently criticised the earlier attempts, and it is therefore unnecessary for me to say anything further about them.

---

[1] Described in Chap. III. of his work, *Mental Development in the Child and the Race*, 3rd ed. London, 1906.

But I was not satisfied with Baldwin's method and it did not seem to me that the results he had obtained by the application of it to one of his children towards the end of her first year, justified even the most tentative conclusions as to the state of her colour-vision at that time.

Baldwin's method consisted in placing small squares of blotting paper of various colours within reach of the child, one at a time, and recording in the case of each colour the ratio of the number of times that the child reached out and seized the presented piece to the total number of presentations of that colour.

The method I adopted may perhaps be regarded as a modification of Baldwin's[1]. I will first describe it and the few results I have hitherto obtained by its means, and will then compare it and these results with Baldwin's.

Although the procedure, as finally practised by me, is very simple, it was arrived at only by means of several steps of refinement introduced in the course of the experimenting. Since less than one third of my results were obtained by what I may call my improved method, and since those obtained by the cruder varieties of the method are not, I think, valueless, it is necessary briefly to describe the successive steps by which the method was improved.

I will use the letters *R*, *Y*, *G*, *B*, *W* and *Gy* to denote red, yellow, green, blue, white, and grey respectively.

My eldest child, L., a girl, was a very lively and vigorous baby. Among my notes made during the early months I find the following:—

12th week, a red ball seems to attract L.'s attention more than others.

15th week, balls of bright worsted of the following colours *R*, *Y*, *G*, *B*, *Gy*, were arranged on a white cloth on the table before L. in semicircle about her; L. reached eagerly after all in turn, but seemed most attracted by *R* and *B*.

In spite of these and other similar observations I noted in the

---

[1] In 1903 Mr R. E. Marsden published an account of an investigation of the colour-sense of his child (*Psychological Review*, 1903, Vol. x. pp. 37–47). He relied chiefly upon Baldwin's method, and obtained by its means results very similar to Baldwin's. He also made some experiments during the child's eighth month by a method essentially similar to the one described in this paper. But the number of these experiments was few (72, apparently); only yellow, red, blue, and green objects were used; and Marsden seems to have regarded Baldwin's original method as preferable to the modified form, although he does not discuss the relative merits of the methods. Marsden's paper, therefore, does not make it unnecessary to describe, and to urge the superiority of, the modified form of the method adopted in my investigation.

16th week that I was not able to draw any positive conclusions as to L.'s colour-sense.

In the 20th week L. sat on her mother's lap in the dark cabinet which I had erected in the garden for the purpose of my own observation of after-images. In this she sat facing a small circular window of ground glass brightly illuminated by the light from the sky. I placed over the window sheets of gelatine of various colours in turn; at the appearance of each new colour L. shewed signs of lively and pleasurable excitement. This seemed to afford strong presumption that the colour-sense was already pretty well developed. I then in the 21st week[1] began to experiment more systematically. I took a white rose and a red rose of similar size and shape, and holding one in either hand behind my back, suddenly brought them up side by side about five inches apart before L.'s face at a convenient distance for her to grasp them. L. put out her hands and grasped one of the two roses. She was allowed to play with it a little, and after a while it was artfully taken from her in such a way as not to excite her displeasure. Then another similar pair of roses was presented in the same way. This simple procedure was repeated so long as L. grasped readily after the flowers. In each case a note was made of the colour taken, of its position to the right or left, and of the hand with which it was grasped.

This was the method in all its essential features. I argued that, if the colour-sense were already developed, it was probable that the coloured object would attract the child's eye and hand more strongly than the colourless object; and that, if in a series of trials, the coloured object was grasped more frequently than the white object, that would afford evidence that the coloured object excited colour-sensation.

I continued the experiments with flowers during the 21st and 22nd weeks. During the first two days, when I used white and red flowers only, L. took *R* 19 times, *W* 9 times.

On the following days I used various red and white flowers, and also blue and white flowers, always presenting as one pair two flowers of the same species differing only in that the one was white, the other coloured. The coloured flower was presented irregularly to the right and to the left of the white flower, but as often in the one position as the other.

In this way I obtained during these two weeks the following results:

---

[1] In June 1901.

TABLE I[1].

R—*W*	*B*—*W*
82—33	17—18

One possible source of error I was aware of from the first and attempted to provide against. I was aware namely that I was pleased when L. took the coloured flower rather than the white one, and that there was therefore some probability that I should unintentionally 'force' the coloured flower upon her. Keeping this danger in mind, I endeavoured to be scrupulously fair; and, in order to satisfy myself that the predominance of colours taken was not due to this factor, I made a series of 26 presentations of red and white flowers, throughout the course of which I deliberately attempted to 'force' the white flower upon L. by slightly favouring it as regards position: the result of this series was—17 *R* taken against 9 *W*.

I believe therefore that the predominance of the colours over white in the total takings cannot be ascribed to any such error on my part in presenting the pairs of flowers.

A second possible source of error I did not appreciate until this first series was completed; it is that, if *W* or any one colour is presented more frequently than others, the chances of it being taken by the child, when presented with one of the less frequently presented colours, are probably diminished, owing to the child preferring the relatively novel and fresh colour-quality. I was able to shew in later series of experiments that novelty may powerfully determine the choice of the child to the relatively novel member of a pair. Nevertheless I do not think that this factor will account for the predominance of coloured over white flowers in the takings of this series; for the excess of white over coloured flowers in the series of presentations was not large; and the predominance of *R* over *W* in L.'s takings was nearly as great in the first part of the series (in which there was no excess of white presentations) as in the whole series.

Having obtained some indications of the presence of colour appreciation in this preliminary series, I determined to continue the experiments using a larger range of colours; and, as I wished to include green and as no green flowers similar in shape and size to flowers of other colours were available, I was driven to use other materials. It was obvious that, if my experiments were to throw light on L.'s appreciation of several colours, a long series of experiments would be necessary and that a principal difficulty would be the prevention of her becoming tired of the game[1]. I therefore used material of three kinds. (1) Balls of worsted of 1 inch in diameter; the colours were rich, *i.e.* of good saturation and brightness. (2) Pieces of crinkle paper (such as is sold in shops for making lamp shades) taken from a series of low saturation. (3) Pieces of similar paper taken from a series of darker and more saturated colours. In every case two pieces of these papers of similar size and folded in similar forms (about 3 inches by 1 inch) were used. I introduced among the white and coloured wools and papers, greys of the same brightness as the colours used as nearly as I could judge; for it seemed probable that the child's choice might be partially determined by differences of brightness (as in fact the experiments proved to be the case).

---

[1] In this and the following tables the two letters within each space denote the two colours presented together; the number beneath each letter denotes the number of times this one of the two colours was chosen out of all the presentations of this pair.

During the following 48 days, falling in the 23rd to the 29th weeks, I experimented on 29 days, at first on almost every day; later, as L. shewed signs of diminishing promptness of reaction, the experiments were interrupted several times for periods of two or three days. Nearly half the experiments of this second series thus fell in L.'s sixth month, nearly half in the seventh month, and a few only in the beginning of her eighth month. Most of the experiments were made out of doors, the coloured objects being illuminated sometimes by sunlight, sometimes by bright diffused daylight. The pair of objects simultaneously presented was in every case two woollen balls or two pieces of paper of similar size and shape but of different colour-tone and, in the cases in which white was used, of different brightness. The tones used were red, blue, green, white, and grey. The three colours were, to the best of my judgment, approximations to the pure or fundamental colour-qualities; they were presented to L. against a background of fairly neutral grey formed by my coat. The colours used in successive experiments were taken up by me at random, the only precaution observed being the avoidance of runs of any one tone. L. sat either in her mother's lap, or comfortably propped up in a basket or mailcart. The experiments on any one day were generally continued as long as L. would readily and spontaneously seize the objects presented. Sometimes they were renewed on the same day after an interval.

---

[1] My later experience shews that this is in fact the chief difficulty in the way of obtaining by means of my method all the light on a given child's colour-sense that the method is capable of yielding.

Occasionally both objects were grasped by L. almost simultaneously, and occasionally she hesitated between them, perhaps touching one and then turning her eyes to the other and grasping it. All such cases were rejected as doubtful. In most cases she grasped the object with one hand only, generally but not always with the hand of the same side as the object grasped. She grasped with both hands in turn, usually with free alternation, the use of the right hand predominating a little, although (as has since become apparent) she is by nature left-handed.

The results of this long series of experiments, amounting to nearly five hundred (exclusive of the failures and doubtful cases), are shewn in the following table:

TABLE II.

R—B	*R—G*	*B—G*	*R—W*	*G—W*	*B—W*	*R—Gy*	*G—Gy*	*B—Gy*	*W—Gy*
30—26	32—16	32—31	36—21	35—15	32—22	30— 4	25— 7	24— 5	38—15

Before making some remarks on these figures, I will briefly describe my experiments with my third child; for, though they were less numerous, the procedure was improved in several respects.

With my second child the favourable time for making similar experiments was lost, owing to some attempts to carry out a much more ambitious scheme of investigation. My experience with all three of my children inclines me to believe that the most favourable age for experimenting with this method occurs about the sixth month, and that the period in which the method can be profitably applied is of short duration, namely some two months only. My experiments with both my first and my third child were brought to an end by the fact that with advancing age both children ceased to be content to grasp one only of the two objects and began to grasp very frequently at both simultaneously with the two hands. As to the earliest age at which the method is applicable—the girl on several occasions took one of two differently coloured flowers presented to her in the early days of the fifth month; and, although I did not begin to experiment regularly until the sixth month, I have little doubt that I might have obtained interesting results throughout the fifth month. But it must be noted that, compared with her brothers and with most of the children described in the books, L. proved herself somewhat precocious in the acquisition of movements.

Owing to the third child's relatively tardy development, I did not hope to be able to carry on these experiments with him before the sixth month, and a childish illness beginning during that month prevented my experiments until the end of the seventh month. It was partly for this reason that I was not able to make so many experiments with the boy; partly also because he grew indifferent to the attractions of my coloured objects more quickly than the girl, so that fewer experiments could be made at each sitting and it seemed necessary to allow two or three days to pass without experiments between the successive sittings. This, I think, was mainly the expression of a native difference between the children, but it may have been partly due to the fact that I used objects of one kind only in the boy's case, and a greater variety of objects in the girl's case.

The improved method.

The objects presented in pairs to this child were balls about one inch in diameter, each consisting of a pill-box containing a pea and embedded in a loose sheath of knitted wool. The woollen thread was prolonged at one point to form a plaited cord about three inches in length. The balls I had made were of red, green, blue, yellow, blue-green, purple, white and grey wools, but I did not get so far with my experiments as to use the blue-green and purple balls. The colours were rich, and the grey was chosen of decidedly brighter shade than the colours, in order that I might be sure that any preference exhibited for the colours was not due to their superior brightness.

I sat face to face with the child, as he sat comfortably propped up in his basket or on his mother's lap with the window behind and above him. I then took two balls from their box, which stood between us on the floor, and, dangling them by their cords, quickly brought them up to a point before his face at which he could easily reach them, keeping them about five inches apart from one another and equally distant from his face[1]. If he did not at once reach out and grasp one of the balls, they were jerked a little, and the rattling then usually drew his attention to them and led to a grasping movement. If he did not take one or both the balls within half a minute, they were removed and a fresh attempt was made after a short interval.

---

[1] In this case again the balls appeared to the child against the background of my coat of slightly yellowish grey tweed. It would, I think, be more satisfactory to present the colours against a large vertical sheet of dull black. I would recommend this addition to any one who may apply this method. The experimenter might stand behind the screen and bring his hands with the balls down in front of it.

This child used the two hands equally freely, nevertheless he would frequently grasp with the same hand time after time. For this reason, and as a general safeguard against my own personal factor playing a part, I wrote down, before beginning each sitting, the pairs to be presented in the order of their presentation. Each pair was presented twice in succession, the positions of the two balls, right or left, being transposed on the second presentation. The ball chosen was underlined in my list. Thus I had at the end of each sitting a complete record in the following form:

Y—$\underline{R}$	Gy—$\underline{B}$	$\underline{G}$—R	Gy—$\underline{Y}$	W—$\underline{B}$
R—$\underline{Y}$	B—$\underline{Gy}$	R—$\underline{G}$	$\underline{Y}$—Gy	$\underline{B}$—W

The writing down of the colours beforehand has the further advantage that it enables one better to avoid undue frequency of presentation of any one colour.

Owing to the causes mentioned above, and in part also to lack of freedom from other occupations on my part, the number of experiments I was able to make with this child (about 180) fell far short of the number I had hoped and planned to make. Nevertheless, since these few results bear out those obtained with the other child, they are stated in the following table:

TABLE III.

R—B	*R—G*	*B—G*	*R—W*	*Y—W*	*Y—R*	*Y—B*	*R—Gy*	*B—Gy*	*G—Gy*	*Y—Gy*
14—18	6—9	0—2	4—0	6—2	6—2	5—8	22—12	27—11	6—8	19—5

The experiments, the results of which are recorded in this table, were made at irregular intervals throughout the eighth, ninth, and tenth months.

We may now consider the significance of the results stated in the three tables. In making the experiments, my first object was to obtain positive evidence of the presence or absence of the colour-sense at the earliest possible age. Since it seems to be generally believed by mothers and nurses that young children are earliest and most strongly attracted by red objects, I began by using pairs of red and white objects. The first short series of experiments seemed to shew that red attracts the child's attention more powerfully than white as early as the end of the fifth month. Since all the later experiments bore out this indication, and since L.'s preference of *R* to *W* seemed at this time to be as strong as, or even stronger than, it was throughout the following months, the whole series affords good evidence that the power of appreciating red was really present and fully developed at the end of the fifth month.

A positive result being indicated in the first experiments as regards red, my next object was to seek evidence that the other colour-qualities are less appreciated than red. Of course, if my experiments had shewn that this was the case, that would not have proved that the sense of red is developed earlier than the others; for the great attraction exercised by red objects might be merely the expression of the greater affective value of red, which seems to be common to men of all races, as well as to many of the animals. I paid special attention to blue in view of the conclusions drawn by Gladstone, Geiger and others, from a study of the language of the Homeric Greeks and other primitive peoples, and especially in view of Dr Rivers's observations[1] on the colour-sense of savages, which seem to lend support to this conclusion—the conclusion namely, that the power of appreciating blue colour or the blue sensation was relatively weak in the Homeric Greeks and is so in many peoples of the lower cultures at the present time. For this conclusion suggests that, as Geiger argued from the philological evidence[2], the blue-sense is a quite recent acquisition, and that in our own race it has come into existence or been further developed during the historic period. Now, if it could be shewn that in the infant blue colour is not appreciated at an age when red, or red and green and yellow, are already appreciated, that fact would very strongly support this interpretation of the philological facts and of Rivers's comparative observations; on the other hand, if blue could be shewn to be appreciated equally well with the other colours, at the earliest age at which any appreciation of colour can be discovered, that fact would to a certain extent make against this interpretation.

The results of the first experiments made with L., those of the 21st and 22nd weeks summarised in Table I, seem to shew that the blue-sense was already present, for the blue flowers were a little preferred to the white. But, while this preference of blue to white was very slight (17 *B*—13 *W*), the preference of red to white was very much more marked (82 *R*—33 *W*). In the week following these experiments, red and blue were presented together 25 times in successful experiments, and of these the red was taken 10 times only, while the blue was taken 15 times; and in the whole of the second series the preference of *R* to

---

[1] *Reports of the Cambridge Anthropological Expedition to Torres Straits*, Vol. II. Part I., Cambridge, 1901.

[2] *Contributions to the History of the Development of the Human Race.*

B, expressed by the results of the experiments in which they were presented together, is very slight (*R* 30—*B* 26). These two sets of figures, considered apart from all the others, would seem to indicate that in L. the blue-sense developed later than that for red, and that in the 21st and 22nd weeks it was still relatively weak, but that in the 23rd week it had overtaken the sense for red. Tempting as this interpretation is in view of the observations of Dr Rivers mentioned above, my figures cannot be taken for more than a suggestion of an interesting line of work for those who may have the opportunity of applying this method to a child of five months or less. The validity of this interpretation is rendered very doubtful by the results of the presentations of *B* and *W* during the 23rd week; for 23 successful presentations gave 12 *B* and 11 *W*—a smaller excess of *B* than in the preceding weeks. These figures suggest that if *R* and *B* had been presented together in the 21st and 22nd weeks no preference for *R* would have been shewn, and that the results are not to be explained by assuming that the sense for blue was imperfectly developed during the 21st and 22nd weeks and that it developed rapidly in the 23rd week, but are rather the expression of the fact that while *R* was not preferred to *B*, it was nevertheless preferred to *W* more strongly than *B* was preferred to *W*; or, more simply, by the more probable assumption that the number of observations was too small to eliminate chance discrepancies.

Putting aside this suggestion of a possible defect of the sensibility for blue in the 21st and 22nd weeks, we may infer from the figures of Table II that in the sixth month the sensibilities for red, blue, and green were well and equally developed; for the objects of all three colours exerted a very much stronger attraction than the grey objects of about the same brightness, and a decidedly stronger attraction than the white objects. Since the white objects attracted L. much more strongly than the grey, it seems clear that brightness was one of the factors determining the attractiveness of the objects; and, since the coloured objects exerted a decidedly greater attraction than the much brighter white objects, it follows that the colours exerted an attraction greater than that of the superior brightness of the white objects,—the colour-attraction was greater than, and able to overpower, the brightness-attraction.

The experiments with the boy A. are relatively few, but the procedure in this case was rather more rigid. It is therefore satisfactory to find that their results bear out the main conclusion deducible from the experiments with L.; for, from the end of A.'s seventh month onwards, the three colours red, blue, and green were much more attractive to him than a grey of distinctly greater brightness, and they were approximately equally attractive (see Table III); and these facts indicate that the sensibilities for red, blue, and green were well and equally developed at this age. In view of the possibility of a relatively late development of the blue-sense, it is perhaps worth while to record that during the eighth month blue and grey were presented together 26 times, and that in these 26 presentations blue was taken 18 times, grey 8 times.

The results obtained with the yellow and white balls present some additional points of interest. Neither of these was used in the first half of the work with A., and when, one day about the end of the ninth month, the yellow ball was presented for the first time in conjunction with the grey ball, he took the yellow so eagerly that I repeated the presentation of this pair eight times. Each time A. looked at the yellow ball at once and seized it promptly, without any hesitation such as he commonly displayed. I then gave *Y* and *R* together four times, and he took *Y* three times and *R* once. (In this series, as throughout the work with A., when the same pair of balls was given more than once in immediate succession, their positions were interchanged.) On the following occasions of experimenting the yellow ball still proved to be more attractive than any of the others, but not in the same degree as on the first day it was given. This pre-eminent attractiveness of the yellow on the first day it was given seemed to be due to its novelty, and this seemed to me to afford the most indisputable proof that appreciation of colour really played a part in determining the child's preference of the one ball to the other. It might be suggested that the yellow ball owed its superior attractiveness to its brightness rather than to its colour; but this interpretation is ruled out by the fact that the white ball was presented for the first time on an occasion later than that on which yellow was so decidedly preferred; yet yellow was then decidedly preferred to white (6 *Y*—2 *W*) on the eight occasions they were presented together. The fact that the absence of the white ball from the earlier half of the experiments did not enhance its relative attractiveness, when it was first presented, in the same way as the attractiveness of the yellow ball was enhanced, was presumably due to the child's familiarity with the white objects and surfaces that had been very abundant in its environment from the time of its birth.

In this connexion I would make the following suggestion:—it may be that the relative attractiveness for the child of the differently coloured objects is determined by two factors, namely, relative brightness and relative novelty. This suggestion is in harmony with the fact that of the three colours, red, green and blue, red seems to have been the most attractive and green the least attractive to L. For, since L., at the time most of the experiments were made and during some weeks before they were begun, spent almost all her waking hours in a leafy garden and under summer skies, and was accustomed to amuse herself for hours together by watching leafy branches swaying over her cot, she must have been most familiar with green and least familiar with red.

I will venture to go further and to suggest that it is just possible that we may find a similar explanation for all the facts which have been advanced in support of the view that the sense of blue is lacking or relatively weak in many savage peoples and that it was similarly defective in the Homeric Greeks (including even Dr Rivers's exact quantitative observations). The superior power of red to strike the attention may be due to the fact that red objects in nature are rare, while blue and green are constantly spread out in large tracts of sky and sea and foliage in the environment of most uncultured peoples. Further, while many of the rare red objects (blood, fruits, animals, ironstone) are of practical importance, and while the abundant green objects are presented in endless variety of tones, shades, and shapes, many of which call for the exercise of discriminatory perception, the widespread blue surfaces of sea and sky are commonly of uniform tone, and in hardly any circumstances is the discrimination of blue tones of practical importance to men of the lower cultures. These environmental conditions are such as might perhaps determine the poverty of terms denoting blue tones, which has been shewn to be common to so many languages, as well as the relative deficiency of the power of discriminating blue of low saturation which, as Dr Rivers has shewn, is common to so many savage peoples.

It remains only to compare my method and results with those of Prof. Baldwin.

As mentioned above, Baldwin's method consisted in putting pieces of paper one at a time upon a rod held at measured distances from the child, and noting the ratio of acceptances (*i.e.* graspings of the paper) to refusals in the case of blue, red, white, green, and brown papers and slips of newspaper respectively. "The newspaper was," says Baldwin, "at reaching distances up to 14 inches, as attractive as any of the colours, and even more so." Although Baldwin suggests that the high ratio of acceptances of newspaper may have been due to these experiments coming late in the series, one might equally well suggest that this condition may have operated adversely, the interest of the child being less keen as the novelty of the situation was changed for too great familiarity. The broad fact remains that the dull white or grey of the newspaper proved as attractive as the colours, and, if this shews anything, it shews that colour as such exerted no attraction.

In another important respect Baldwin's results differed from mine. He found that "the colours range themselves in an order of attractiveness, *i.e.* blue, red, white, green, and brown[1]." But the number of experiments with white was only eleven, and, though the figures would, perhaps, suggest some colour appreciation if the experiments had been more numerous, they hardly can be claimed, as they stand, as evidence in that direction. The only other feature of Baldwin's results that seems capable of being regarded as evidence of the presence of colour-vision was the low attractive power of brown. But this may well be attributed to the relative lack of brightness of brown—for brightness is certainly one factor of the attractiveness of the object. It seems also not impossible that the background on which the colours were presented was relatively unfavourable to brown; for Baldwin says nothing on this important point, save that the colours were placed upon a rod.

It seems, then, to me that the results actually obtained by Baldwin cannot be held to shew that colour-vision was present, or to throw any light on the state or of development of the colour-sense in his subject[2].

---

[1] *I.e.* as measured by the ratio of acceptances to presentations of each colour. The table gives the ratios $\frac{\text{acceptances}}{\text{total presentations}}$ as follows: blue ·766, red ·714, white ·686, green ·60, brown ·50.

[2] The remark applies, I think, equally well to Marsden's results; for, although, of course, the fact that his results are concordant with Baldwin's, lends support to the interpretation of the figures as evidence of the children's colour preferences, the actual differences of frequency with which the various colours were taken seem hardly large enough to fall outside the margin of error. The figures are as follows: newspaper 130, yellow 127, red 122, blue 121, white 118, grey 114, black 114, brown 107 (the figure denotes in each case the number of times the object was grasped in the course of 175 presentations). The extreme difference (that between the reactions to newspaper and those to brown) is thus only about 18 per cent. And, if we should nevertheless incline to accept them as indications of the child's preferences, other difficulties arise: *e.g.* white, grey, and black were taken equally frequently, therefore brightness has no influence in determining preference; yet of the colours yellow was taken most frequently, and brown (which is commonly regarded as

And, though evidence might perhaps be obtained by means of Baldwin's method, it would probably have to take the form of a concordance of the observations made upon a number of children; for it is, I think, doubtful whether the number of observations that could be made with any one child would suffice to establish any conclusion. With the modified form of the method used in my experiments the principal difficulty is to secure a sufficiently large number of results from any one child, during the period when such results are of interest[1]; and it is, I think, clear that with Baldwin's method a considerably larger number of experiments must be made and that fewer can be made at each sitting. For the uncontrollable factors, both internal and external, that co-operate with the colour and brightness of the object in determining refusal or acceptance of any object by the child are very numerous and are very variable from moment to moment and from day to day; *e.g.* the least degree of weariness entirely vitiates the results of experiments made by Baldwin's method, so that, as Baldwin says, "No series longer than three trials should be attempted without changing the child's position, resting its attention with a song or a game, etc." On the other hand, by the device of presenting the two coloured objects simultaneously all these many variable factors are rendered innocuous for the result; *e.g.* the child may be indisposed, or fatigued, or bored to any indefinable extent, but so long as he takes one of the two objects presented, the result is valid and significant[2]; whereas the least degree of any one of these conditions goes far to render valueless any result obtained by Baldwin's method during its prevalence.

A further serious difficulty of Baldwin's method, to which he makes no reference, is connected with the duration of each experiment. Clearly the presentation of the coloured paper cannot be indefinitely prolonged until the child grasps it, and one would like to know what constituted a 'refusal' in Baldwin's view; for 'refusal' means merely failure to grasp within some undefined period; and this difficulty cannot be satisfactorily surmounted by fixing some arbitrary limit to the

---

but a dark shade of yellow) was taken less frequently; and the preference of newspaper (if preference is expressed by the fact that it was the most frequently taken) must be due to some other factor than colour or brightness.

[1] It is obviously desirable to make the largest possible number of significant experiments within the few weeks immediately following the date at which experiment becomes at all possible.

[2] With my method accordingly a large number of experiments can sometimes be made at a single sitting in rapid succession. I commonly made about ten or twelve experiments at a single sitting and, on some days, when L. was in the humour for the work, I made with her more than forty experiments grouped in two or three sittings.

duration of the presentation; for suppose any such limit to be adopted, say thirty seconds, then it might often happen that during this period the child's gaze and attention would never be directed towards the object. With the modified method, on the other hand (though it is well to adopt some limit of duration of presentation), no such difficulty arises; for though the presentation of two objects were prolonged for many minutes or even an hour or more, the result would be significant, if the child eventually grasped one of them.

For these reasons chiefly, and also because the number of my experiments was considerably greater than the number of Baldwin's[1], and because their results were of a more positive character (although the subject of the greater number of my experiments was some three months younger than Baldwin's subject) I venture to think that the conclusions indicated by my experiments are more worthy of a tentative acceptance than those drawn by Baldwin from his experiments, and that my modification of the method renders it capable of throwing light on the state of the colour-sense of a more positive and reliable character and at an earlier age than any that can be obtained by the original method of Baldwin.

Summary of principal Conclusions.

(1) The proposed modification of Baldwin's 'dynamogenic' method of investigating the colour-sense of infants renders it capable of affording evidence of the state of the colour-sense from the sixth month onwards and perhaps even during the fifth month.

(2) The experiments made indicate that red, green, and blue are appreciated during the sixth month, since they are decidedly preferred to white and still more decidedly to grey of equal brightness with the colours.

(3) The experiments indicate that in the sixth month no one of these three colours is markedly preferred to the others; but there was a faint indication that during the fifth month blue is less appreciated than red.

---

[1] The total number of Baldwin's experiments was 217, of which 106 were with bits of newspaper; the number of mine, in all of which (save the few in which white and grey were presented together) red, green, blue, or yellow was presented, amounted to more than 600 successful experiments with L. and nearly 200 with A.

SOME OBSERVATIONS ON THE DEVELOPMENT OF THE COLOUR SENSE.

BY CHARLES S. MYERS.

1. *A new Method of Experiment.* 2. *A Method of Grasp and Reward.* 3. *Experimental Data.* 4. *Advantages of Second Method.* 5. *Legitimate Conclusions from these experiments.* 6. *Illegitimate Conclusions.* 7. *Conceptions of the Development of the Colour Sense.* 8. *The Significance of Observations on the Colour Sense of Primitive Peoples.* 9. *General Conclusions.*

I PROPOSE to give an account of some observations made by me during the years 1905—6 on my elder child. They are less numerous and were, for the most part, made somewhat later in the child's life than those recorded by McDougall in the present number of this Journal. Indeed, were it not for the appearance of his very suggestive paper, I should not have thought it worth while to publish them as they now stand. They are of interest, however, inasmuch as they independently confirm several of McDougall's conclusions, and enable me to describe a method which is in some respects, I venture to think, preferable to the method employed by Marsden and him. Moreover, finding that I differ widely from these and from other observers as to the justifiability of deducing from their data the order of development of colour sensations in infants, I take this opportunity of expressing my views on this subject in particular, and on the development of colour vision in general.

1. I prepared for my experiments a number of wooden cubes ('bricks') each measuring 33 × 20 × 15 millimetres, and I painted each of them uniformly in a different shade of grey or colour. I placed a pair of these bricks before the infant upon a table covered with a piece of black velvet; the table was placed near a window, and the infant sat comfortably before the table on her mother's lap with her

back to the window. A black screen was interposed while the bricks were being placed in position so as to hide them momentarily from the infant's view. In all the experiments save a few mentioned on p. 357, the two bricks were placed one to the right, the other to the left of the infant about 15 centimetres apart. They were placed at such a distance from her that she could easily grasp either of them without effort.

I started these experiments in the hope of being able to employ a method which, so far as I am aware, has not been hitherto applied in observations on infant behaviour. I began by using two coloured bricks, one red, the other blue. Each time the infant picked up one of them, say red, she was rewarded by being given a taste of honey, syrup or sugar. When she picked up the other (blue) brick, no reward was given her. In this way I hoped to build up in the infant a definite association between red and the reward, so that she would always tend to select the red brick when it was offered simultaneously with any other (coloured or colourless) brick. I purposely chose a red brick for the construction for this association, as the natural preference of children for this colour is so well marked.

By this method I hoped to be able to show what colours tended to be confused with red. If, as has been generally supposed, the blue sensation is lacking in early life, it was quite conceivable that the infant would have reacted to a purple or carmine in the same way as she would have done to a red of the same brightness. She would have been insensible to the blue component of these colours and would have regarded them as red.

My first attempts at experiment were made on October 12, 1905 during the twenty-fourth week of the child's life. They were continued on Oct. 14, 16, 17, 27 and 28. About twelve pairs of bricks were presented each day. I take the following remarks from my note book:

October 14. She does not appear to look at the bricks as a rule before grasping them. Her arm movements are usually made without looking, they sweep over the bricks, occasionally picking up one. On rare occasions she puts the brick to her mouth.

October 16. The same sweeping arm movements occur, the hand grasping the object on coming into contact with it; the gaze subsequently fixed upon the object grasped which is then moved towards mouth. The infant has no clear notion of the source of the honey, and seems quite unable to connect grasp and reward.

October 27. Still no trace of association between grasp and reward.

October 28. She no longer makes sweeping movements and begins to look before picking up the brick.

As there was not a trace of the desired association on this date, the experiments were now interrupted. They were begun again at the end of December, 1905 (the thirty-fifth week of the infant's life), and were continued with fair regularity until June 4, 1906. By this time I had abandoned the above method, because I foresaw great difficulty in determining whether the grasping of any brick other than red was due to the confusion of its colour with red or to the attractiveness of that colour arising from its novelty. Now, however, this difficulty does not seem to me insuperable; for, if an infant were really blue-blind, it should, *ceteris paribus*, more often take up carmine than, say, orange instead of red. I have described the method in the hope that future observers may find it worth while to adopt it and to record their experience. It differs from all other methods with which I am acquainted in that it can claim to be a test for colour confusion. In this respect it has the great advantage of being comparable to the methods commonly used for the detection of colour blindness in adults.

2. Between January 14 and May 3 I investigated the sensibility of my child to differences of brightness, using a pair of grey bricks, one lighter than the other. The lighter brick matched a grey compounded on the colour wheel of 129° white and 231° black; in the case of the darker brick the proportions were 50° white and 310° black. This pair of grey bricks was presented about ten times at each sitting, sometimes the lighter being on the infant's left, the darker on her right, at other times these positions being reversed. The order of these changes in position was always arranged and written down before the beginning of each sitting. Whenever the infant picked up a brick, she was rewarded with a taste of honey. On January 22 I noted that whenever the brick was taken from her hand, she opened her mouth and projected her chin, clearly showing that she expected the spoon to approach her. On some days subsequently, *e.g.* on January 31, February 6, 7 and 27, she seemed disinclined to observe the bricks; she showed no signs of expecting the reward, and appeared so little interested and so inattentive that the experiments had to be discontinued. On February 18 my note runs, "very good to-day, certain expectancy of spoon after exhibition of the second pair of bricks," and

on February 19 "she certainly expected sugar from the first, occasionally dropping the brick of her own accord and at once looking for the sugar." On March 8 she showed disinclination for sugar which was accordingly replaced, with success, by golden syrup.

3. Whenever a brick was picked up, note was taken of its shade and of the hand (right or left) that picked it up. If both hands were put forward, the object recorded as chosen was that which she grasped first. The following results were obtained:

Shade of brick	Number of times selected
Light-grey	101
Dark grey	75

Having thus demonstrated the effect of relatively small differences of brightness on the infant's choice of a brick, I proceeded on May 7, 9, 10, 14 to use a vivid yellow brick and an intensely white brick, presenting them in pairs as before. I obtained the following results:

Brick	Number of times selected
Yellow	27
White	8

These figures show very clearly the superior attractiveness of yellow, when presented with white, in spite of the greater brightness of the latter.

On May 15 I began to use a saturated-blue brick and a white one. In eight choices the blue was always picked up, the white never. So far, the results were even more striking than those obtained from the yellow and white bricks. At the next sitting, however, on May 24, the blue brick was chosen only seven times, the white thrice, in ten trials. Continuing with the use of these blue and white bricks on May 29, 30, 31 (twice this day), June 4 and 5, I found that the blue brick was picked up 35 times and the white 43 times in a total of 78 trials.

These results are interesting as they show the influence exerted by novelty of colour. In the first two experiments the blue brick was obviously preferred on the score of novelty, whereas subsequently the blue and white bricks were picked up almost equally often.

On the other hand, in the experiments with the yellow and white bricks, the yellow was chosen with increased instead of with diminished frequency at successive sittings. The preference for yellow differs, then, from that for blue, in being independent of the factor of novelty.

During my earliest experiments made in October 1905, I noticed that the infant unquestionably preferred her left hand to her right, the frequency being in the proportion of about five to one. From January onwards, the use of the right hand began to predominate. This is shown in the course of the three groups of experiments just described. Each number in the following table expresses the percentage frequency, the bracketed numbers express the absolute frequency, with which the object was grasped by one or other or by both hands.

Age of Infant	Brick	Right hand	Left hand	Both hands
37th—53rd week	Light grey	44·5 (45)	13 (13)	42·5 (43)
	Dark grey	42·6 (32)	21·3 (16)	36 (27)
53rd—54th week	Yellow	49 (13)	44 (12)	7 (2)
	White	63 (5)	25 (2)	12 (1)
54th—58th week	Blue	80 (40)	18 (9)	2 (1)
	White	70 (32)	26 (12)	4 (2)

This table shows very clearly the increasing predominance of grasps by the right hand. It also shows a reduction in the number of grasps by both hands,—an observation directly opposite to that recorded by McDougall (p. 343). It is of interest to note that on each of the five occasions on which the white brick was grasped by the right hand in preference to yellow, it lay to the right of the yellow brick, and was therefore more readily accessible to the infant's right hand than the yellow.

These results show very clearly the importance of repeatedly interchanging the position of the members of each pair of objects in regard to one another. On May 21 and 22 I escaped from this influence of the spatial error by placing the two bricks in the medial plane, one occupying a position four inches nearer the child than the other. On these two days three bricks were used, the same saturated blue, the white and the previously mentioned dark grey brick, each brick being presented with one of the others equally often. The result of twenty-four trials was as follows: right hand grasps 17 times, left hand grasps 7 times: when the blue and white bricks were presented together, they were picked up equally often (4 times): when the blue and grey bricks were presented together, the blue was picked up 5 times, the grey 3 times. That is to say, the same preference for hand and the same indifference for blue were observed as have been already described.

4. The advantage of my second method over that of Marsden and McDougall lies in its perfect freedom from the effects of suggestion.

These observers, it will be remembered, allowed two woollen balls to dangle from their hands before the eyes of the infant. In such procedure there is every opportunity for the play of suggestion. The experimenter can hardly avoid expecting and desiring the infant to choose a ball of particular colour, and accordingly he is apt (no doubt quite unconsciously) to dangle that ball with greater vigour. In the hands of a trained observer, the method may be quite free from this objection. But when it is adopted by others who are devoid of scientific training, the dangers of suggestion become very considerable.

My 'method of grasp and reward,' as it may be termed, escapes these dangers. It works perfectly well from the child's ninth month onward. Its applicability to still younger children may appear somewhat questionable. It is true that in my six months old child I was unable to build up an association between a *particular* colour (red) and the reward of sugar (p. 354). But that does not warrant the conclusion that at this age I could not have got the child to expect a reward each time it picked up *any* brick. On the contrary, I have not the slightest doubt that this second method, the method of grasp and reward, is available for infants below the age of nine months.

Only one coloured brick and a grey (or white) one were exhibited to the infant at any one sitting in my method. This appears to me preferable to using combinations of various colours (and greys) indiscriminately at each sitting, as the disturbing effects of novelty of colour are better controlled. Moreover by using the same two bricks throughout a sitting, I was able to escape the likely drawbacks of McDougall's procedure in which each pair was presented *twice in succession,* their positions, right or left, being transposed on the second presentation.

5. From the experiments which McDougall and I have here published, I believe that we are justified in concluding (*a*) that at a very early age (probably long before the sixth month) infants are susceptible to relatively small differences of brightness, (*b*) that at this age reds and yellows are distinctly preferred to other colours and to colourless objects of far greater brightness, and (*c*) that novelty may play an important part in determining the infant's choice of colour.

6. Thus far, I am in agreement with McDougall and with several others who have drawn conclusions from their investigation of the *behaviour* of infants to colour. But I wish to enter a very strong protest against the endeavour to trace, on the basis of these experiments, the *course of the development of the colour sense. I am convinced that it is extremely dangerous to formulate any opinion on the actual colour*

experiences of an infant as the result of observing what coloured objects it prefers or rejects, when these objects are presented with other coloured or colourless objects.

It is true that the positive results of such experiments may be significant. When, for example, a child shows a distinct preference for yellow, presented with white, that is a clear indication that yellow has a different effect on him from white. And we are doubtless justified in assuming that this difference is not merely an affective and physiological one, producing greater pleasurable excitement in the infant and determining the choice of the brick which is grasped, but that it is also of sensational significance, that is to say, the visual sensation excited by yellow is different in the infant from that excited by white.

When, on the other hand, we consider the negative results of these experiments, we may be led into drawing conclusions which are demonstrably false. Let us suppose that an infant is presented with successive pairs of objects, each pair consisting of a blue and of an equally bright grey object, and that he is observed to choose the grey as often as he chooses the blue (cf. p. 357). Are we justified in concluding from this observation that the child is colour-blind to blue or that his sensation of blue is in any way different from the blue-sensation of an adult? Surely not. All we can say is that blue does not attract the infant as powerfully as certain other colours do, notably reds and yellows. Supposing that this infant showed a preference for the taste of syrup when he had to choose between syrup and lemon juice, and supposing that he showed no preference when he had to choose between lemon juice and beer, who would venture to conclude that the sweet taste was already developed in the infant but that the sour and bitter tastes were not yet differentiated from one another? Yet conclusions on precisely these lines are drawn by those who have in a similar manner investigated the colour preferences of infants. McDougall, for example, remarks of his child L. that the results may "seem to indicate that in L. the blue-sense developed later than that for red, and that in the 21st and 22nd weeks it was still relatively weak, but that in the 23rd week it had overtaken the sense for red" (p. 347). From such an interpretation I strongly dissent. The behaviour of infants in these experiments teaches us only that certain coloured objects are *more attractive* than other colours or than the colourless (white, grey and black) series.

7. There are various reasons which to my mind make it improbable

that the infant becomes sensitive to different colours at different times in his life. Naturally, those who uphold the Young-Helmholtz theory of colour vision are eager to prove that first the infant becomes sensible of red, next of green and finally of blue. They appear, however, to forget that this same theory makes the sensation of white dependent (under ordinary conditions) upon the excitation of all three of these fundamental colour processes, and that if at any period in his life the child were colour-blind to green and blue, all objects, coloured and colourless, could only appear as shades of red. Or, to take a less extreme case, if we suppose that an infant is at a certain stage deficient in sensibility of blue, we can hardly avoid the inference that as in course of time this sensibility becomes greater, his experience of white changes *pari passu*.

Were it not for the demands of theory, who would suppose that the colour sense arrives at maturity in these stages? Who would suppose that the infant becomes sensitive first to the colour of the longest waves of light and later becomes more and more sensitive to the colour of the shorter waves? Would any one, turning to the sense of hearing, hold that the infant is first conscious of the pitch of low tones and later of the pitch of the highest? It might indeed be urged, on the grounds of the infant's readier response to high tones (and of the pitch of his cry), that the order of development is just the reverse.

Surely it is more likely that vibrations of whatever frequency, so long as they affect the infant's retina and cochlea at all, at first evoke merely sensations of light and sound, and that from this primary experience of light the coloured and colourless series of sensations become gradually and simultaneously differentiated, while from the primary experience of sound tones of various pitch and noises are similarly differentiated from one another. It seems to me most improbable that, in the development of the individual or of the race, sensations of one colour region or of one tone region develop before those of other regions. I look on the process of sensory development as a gradual unfolding, a gradual appearance of diversity among experiences which earlier seemed identical.

We ought, I think, to be on our guard against confusing the psychic experience of the individual with the peripheral sensory apparatus with which he is endowed. Whatever theory of colour vision we hold, to whatever degree the sensory apparatus, on which that theory is based, be developed at a given age, it seems to me that the functional activity of that apparatus may at any moment be prevented from fully manifesting itself by a temporary insufficiency of experience. I think it highly probable that the primary physiological basis of colour vision is completely installed before the infant has reached the stage when he can successfully differentiate from one another all the various colour sensations which such an apparatus permits him to receive; just as, in foetal life, he is provided with lungs before he can make use of them. If this be true, then it follows that the gradual differentiation of the colour sensations from one another is a process distinct from the developing constituents of the peripheral cerebro-retinal apparatus.

Those who interpret the developing colour sensations of the infant in the light of the Young-Helmholtz theory, may be said to regard the infant as if he were learning to photograph in colours by the three-colour process. At first he has only the 'red plate' to work with, and at this stage red is the sole colour sensation he receives. Later he acquires the 'green' and finally the 'blue plates.' He is thus at length able to reproduce the colours of the external world faithfully. I am here endeavouring to show that the infant's earliest vision is with more likelihood comparable to the images projected on to the ground-glass screen of a camera, the lens of which is racked quite out of focus. All then is blurred and confused; there are no distinct outlines and no distinctions of colour. I contend that the growing experience of the infant is comparable to the gradual adjustment of the lens. Little by little, colours begin to be simultaneously differentiated from one another. What was at first a homogeneous light is later seen to consist of differently coloured parts.

8. In favour of the late acquisition of the sensation of blue, it is often urged that "the power of appreciating blue colour or the blue sensation was relatively weak in the Homeric Greeks and is so in many peoples of the lower cultures at the present time." This view, which McDougall appears to quote (p. 346) with approval, rests mainly on the absence of a specific word for blue from the vocabularies of these peoples. But language affords no safe clue to sensibility. A colour name occurs when it is needful. Where it is needless, it will not be formed, be the sensibility to that colour ever so great. If we are to gauge the colour sense of a people by colour nomenclature, nearly every primitive people must be dubbed 'brown-blind' or 'brown-weak,' inasmuch as it is very rare to find a special word for brown. Even when the vocabulary of a primitive people contains a separate word for blue (distinct from the word for black), that word is frequently used for green as well as for blue. Indeed blue, green and yellow may share

a common colour name. It does not follow, however, that such peoples confuse blue and green or that they cannot distinguish blue from green and yellow.

I do not believe that this absence of a definite word for blue among the Homeric Greeks and modern primitive peoples is necessarily connected with Rivers's observation that the threshold of discrimination between white and the palest blues is higher among coloured races than among Europeans. This difference is probably related to racial differences of pigmentation of the macula lutea, as Rivers has himself suggested[1]. It has only been found among coloured peoples and consequently we have no right to suspect its presence among the ancient Greeks and primitive peoples of the west, who also possessed no distinct colour name for blue.

9. I conclude, then, that we have not sufficient evidence to show that the colour sense materially differs in different peoples, or that the various colour sensations of an infant develop at different periods in his life. The 'red-blindness,' which occurs among the colour-blind, and in the intermediate zone of the normal retina, is strong evidence against the view that red is the first colour-sensation phylogenetically acquired. The one prominent fact, which stands out clearly in this discussion, is that both primitive peoples and infants are attracted most by red and next by yellow; this fact being manifest among the former in their colour nomenclature and among the latter by their readiness to grasp objects of these colours. I suggest that this superior attractiveness of red is a fundamental characteristic far too deeply and immutably ingrained to be attributable, as McDougall so ingeniously suggests, to the greater utility or rareness of red and yellow objects or to the relative unattractiveness of the broad surrounding expanse of uniformly blue sky, blue-green sea and green foliage. The excitatory action of red is manifested in organisms lower than man.

---

[1] *Reports of the Cambridge Anthropological Expedition to Torres Straits*, Cambridge, 1901, vol. II. pp. 79, 80.

THE INFLUENCE OF BINAURAL PHASE DIFFERENCES ON THE LOCALISATION OF SOUNDS[1].

By H. A. WILSON and C. S. MYERS

(*from the Laboratories of King's College, University of London*).

A. Experimental. 1. *Introductory.* 2. *Experimental methods and general results.* 3. *Effects produced by forks of different pitch.* 4. *The buzz.* 5. *Anomalous effects due to resonance.* 6. *The 'inertia' of judgment.* 7. *Influences of adaptation.* 8. *The lateral error.* 9. *Summary of factors determining individual differences of judgment.*

B. Theoretical. 10. *The difficulties of the theory that the phases of sound vibrations are directly transmitted to the brain.* 11. *Our alternative explanation.* 12. *The intra-cranial conduction of tones from ear to ear.* 13. *The retardation in phase during intra-cranial conduction.* 14. *The meeting of the direct and the bone-conducted waves.* 15. *Its application of our explanation to binaural beats and to the localisation of low tones under ordinary conditions of hearing.* 16. *General conclusions.*

A. *Experimental.*

1. A brief notice of the experiments which we describe in this paper has already been published[2]. They were begun in February 1907, having been suggested to us by Lord Rayleigh's account[3] of his investigations in the same field of research. We wish now to present our data in fuller detail, giving special consideration to those results which are of psychological interest, and to those which throw light on the localisation of low-pitched tones under ordinary conditions of hearing.

At the outset we proposed to ourselves the following problem. If the two ends of a long curved rubber tube be inserted one into each ear of an observer, what changes will occur in the apparent position of the sound of a vibrating tuning-fork, as the fork is moved (with its stem always touching the rubber tube) from the middle point of the tube towards one ear or the other? Have the differences of phase between the vibrations reaching the two ears any influence upon the apparent direction of the sound?

---

[1] We wish to thank Lord Rayleigh for the interest he has taken in these experiments and for several suggestions which he has offered us. We would also express our obligation to the Drapers' Company, by whose liberal grant to the Physical Research Laboratory of the College the expenses involved in this investigation were defrayed.

[2] *Proc. Roy. Soc.* 1908, A. Vol. LXXX. pp. 260–266.

[3] *Phil. Mag.* 1907, Ser. 6, Vol. XIII. pp. 214–231, 316–319.

We were not then aware that thirty years ago a similar problem had been made the subject of a very rough experiment by Sylvanus Thompson[1]. This investigator introduced the ends of a curved copper wire, 3 feet long, into the ears of an observer, and set the stem of a vibrating tuning fork on the middle point of the wire, whereupon "the sound...appeared to come from the ends of the wire in the ears." He then slid the stem of the fork along the wire through a distance of about an inch and a third, whereupon "the sound appeared to come, not from the two ends of the wire, but from the back."

Under the improved experimental conditions which we are about to describe, we have been able to trace the influence of the differences of phase between the vibrations reaching the two ears in a more thorough and successful manner than was done, either in the above-mentioned experiment by Thompson, or in the more careful experiments which were published by More and Fry[2] while our investigations were in progress. Probably owing to their mode of procedure, all three observers failed to demonstrate the striking series of changes in localisation which are to be met with,—changes of localisation from the middle of the head, across to one ear, back to the middle of the head and across to the other ear, as the tuning-fork is gradually moved from a symmetrical position nearer and nearer to one and the same ear.

2. Instead of using a rubber tube, we joined together pieces of brass and glass tubing of the same calibre by means of very short pieces of equally wide rubber tubing. And instead of moving the fork along the tube, we introduced a hollow brass T-piece, the horizontal arm of which could just slide freely within two slightly larger tubes, its short vertical arm receiving the sound of the tuning-fork. This arrangement is made clear in Fig. 1, where the (partially dotted) tube *ATB* is the T-piece sliding within the closely fitting brass tubes *SC*, *VF*. From the points *C* and *F* the sound was conducted by glass tubes, joined as we have just described. These tubes were supported on wooden stools and ended in the ear caps, *P* and *Q*, which consisted of wooden discs lined with

---

[1] *Phil. Mag.* 1878, Ser. 5, Vol. VI. pp. 386, 387.

[2] *Phil. Mag.* 1907, Ser. 6, Vol. XIII. pp. 452–458.

annular soft pads so that they could be pressed against the observer's head *H*. The ear caps were supported on the retort stands, *M* and *N*, which were clamped to a large table. The length of each of the arms, *SCP*, *VFQ*, in most of our experiments, was 317 centimetres, their diameter was approximately 2 centimetres. As a rule, the observer sat facing the tuning-fork; but a large wooden screen, *SS*, was interposed between the head of the observer and the T-piece, so that the position of the latter was completely hidden from him. A graduated scale *DE* was fixed beside the T-piece, so that the position of the point *T*, *i.e.* the point of entrance of the sound from the tuning-fork *K* could be accurately recorded. It will be seen that the point of entrance of the sound could be varied through a range of 60 centimetres, on either side of the middle point 100.

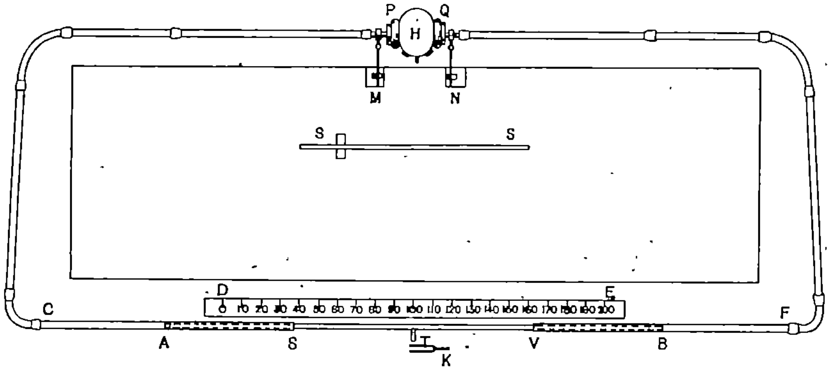


FIG. 1.

By aid of this apparatus, we have been able to show that the result of slightly moving the point *T* from the middle (symmetrical) position, say towards *V*, is to shift the apparent position of the tone of the fork from the middle line to that side of the head, *Q*, towards which the T-piece is moved. And we find that, as the T-piece is moved still further towards *V*, this lateral displacement of the tone is reduced; a position of *T* is reached when the tone is again localised midway between the two ears. If the T-piece be again moved nearer to *V*, the tone passes over to the opposite ear *P*; and if it be still further moved in the same direction, the localisation of the tone again changes, passing through every gradation until it is localised once more at *Q*.

These series of changes in localisation can be repeated several times, as the point *T* approaches the point *V*, the number of the reversals depending on the wave length (λ) of the tone employed and upon the available distance SV. As the point T is moved towards V, the tone is found to be localised on the observer's left (*i.e.* at Q) when the distance from T to the middle point of the scale (100) lies either between 0 and $\frac{\lambda}{4}$, or between $\frac{\lambda}{2}$ and $\frac{3\lambda}{4}$, while it is localised on his right when this distance lies either between $\frac{\lambda}{4}$ and $\frac{\lambda}{2}$ or between $\frac{3\lambda}{4}$ and λ; the same holds for simple multiples of λ. The converse effects are observed when the point T is moved towards S.

In all these experiments one of us acted as observer, while the other varied the position of the T-piece, sounding the tuning-fork at each position and asking the observer where he localised the tone. On two occasions only we obtained the help of two other subjects, in order to confirm our results. We shall presently give a more detailed account of the experiences of the observer at different positions of the point T. For the present it is sufficient to say that we found it possible to grade our answers in the following manner, 'full-right' (R), 'half-right' $\left(\frac{R}{2}\right)$, 'middle or half-right' $\left(\frac{R}{4}\right)$, 'middle' (M), 'middle or half-left' $\left(\frac{L}{4}\right)$, 'half-left' $\left(\frac{L}{2}\right)$, 'full-left' (L) according to the definiteness of the lateral or medial localisation. The following may be regarded as a typical series of observations:

Fork 884. Observer with back to fork.

Scale reading	Localisation	Scale reading	Localisation
100	M	75	R
120	R	80	M
140	M	90	L
160	R	80	L
150	$\frac{R}{4}$	135	M
130	$\frac{L}{4}$	125	R
110	R	90	L
90	L	85	M
80	L	80	R
70	R	80	L
60	R	75	R
50	L	80	L
40	L	120	R
55	M	140	M
70	R	130	$\frac{L}{4}$

Such results we have conveniently expressed in the form of curves (Figs. 2—6), the coordinates of which are (i) the scale readings of the

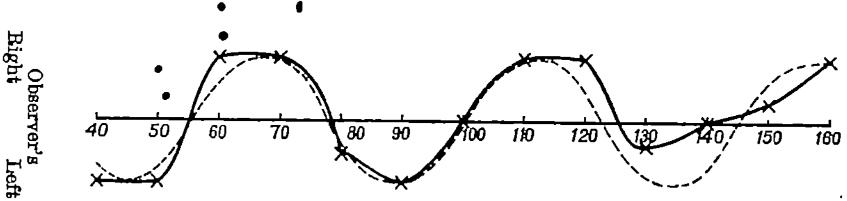


Fig. 2. Observer with back to fork 384.

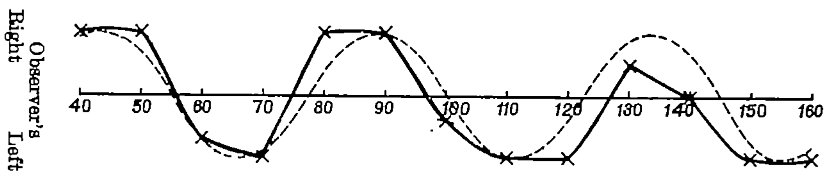


Fig. 3. Observer with face to fork 384.

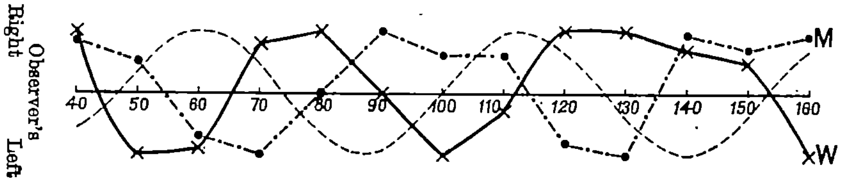


Fig. 4. Observers with back to fork 320.

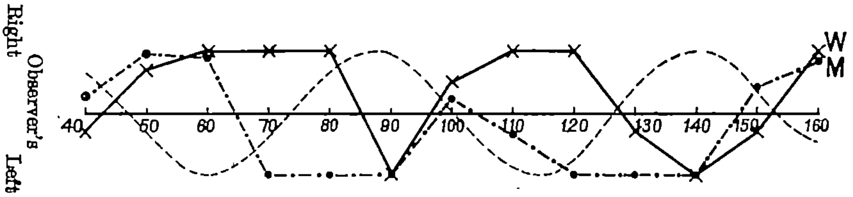


Fig. 5. Observers with face to fork 320.

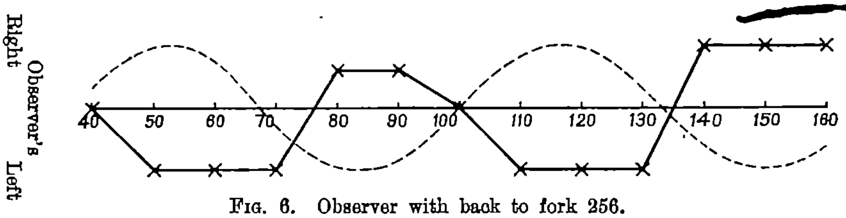


Fig. 6. Observer with back to fork 256.

position of the T-piece and (ii) the localisation effects. We allow a 'full-right' to count as 1, a 'half-right' as $\frac{1}{2}$, a 'middle or half-right' as

$\frac{1}{4}$ and a 'middle' as 0; and we use corresponding negative numbers to express left effects. The curve shown in Fig. 2 has been constructed from the data for the fork 384 just given. The dotted line shows the curve we should have expected, had the observations precisely followed the rules we have laid down, that is to say, had the lateral effect ϕ been determined solely by the expression $\sin(4\pi x/\lambda)$; where λ is the wave length of the tone, and x is the distance in centimetres of the point T from the middle point (100) of the scale. It will be observed how closely the experimentally and theoretically derived curves agree. Into the causes of the deviations we shall enter presently.

Besides the fork 384, we also experimented with the forks 128, 180, 240, 256, 320, and 512. We were unable to obtain any changes in lateral effect in the case of the high tones emitted by König's steel rods. On a few occasions the forks were driven electrically, but we found it far more convenient for the experimenter to strike the fork. The strength of his blow could after a little practice be kept constant enough for the observer not to be disturbed by differences in the loudness of successively given tones.

Usually about thirty-five observations were taken at one sitting. The point T of the T-piece was usually placed at 40, 50, 60, 70, etc., and occasionally at intermediate points 45, 55, 65, etc.[1] The order in which the various positions of the T-piece were taken varied in the course of our experiments. At the outset of our investigations the order was quite irregular, but at every sitting each position of the T-piece was usually given at least twice. Later, the positions were taken in irregular order, but at half-time they were repeated in exactly the reverse order. Sometimes a procedure of complete ascent and descent was adopted, the T-piece being regularly moved from end to end of the scale. Other orders were occasionally used for the determination of special influences (page 373).

We have already given the data and the curve deduced therefrom in the case of a set of observations with the fork 384. In the short abstract already mentioned, similar curves will be found which were obtained from the forks 128 and 512. We reproduce (Fig. 3) the curve there given for the fork 384, which is almost an exact reversal of that which we have already produced (Fig. 2). In the latter, it will be remembered, the observer had his back to the screen and tuning-fork;

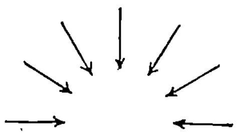
---

[1] For the sake of convenience, the few data obtained at these intermediate points have been omitted in the construction of the curves.

whereas in the experiment which gave this curve (Fig. 3), the observer faced the screen and tuning-fork.

As a rule the localisation of the given tone presented little difficulty. Even at the outset of our investigation, it was obvious that at some positions of the T-piece the tone produced a much stronger lateral effect than at others. At his first sitting, for example, one of us (W.) gave answers 'well-marked right,' 'right,' 'query right,' 'doubtful,' etc. After a little practice, the 'doubtful' answers became extremely rare. We soon settled down to grading our replies in the manner already described on page 366.

3. One of us (M.) endeavoured to study the differences between different forks introspectively. With forks 128 and 256, the observer facing the screen, the tones appeared to reach the head in one or other of the directions shown by the arrows. When the tone was judged to be medial, it appeared to come sagittally through the forehead. When it was judged to be markedly lateral, it appeared to come coronally across the head. Intermediate positions of localisation (*e.g.*, 'middle or half-right,' 'half-left') corresponded to the intermediate directions of the arrows. The fork 384, on the other hand, never gave such a well-marked right or left effect. Occasionally a tone from this fork was localised as 'middle,' which appeared to be absolutely without position; the judgment was determined only by inference from the fact that the tone seemed quite devoid of any lateral character. Lastly, the effects of the fork 512 were observed. This fork was obviously localised with greater ease than the fork 384; the answers came without any hesitation. All the lateral effects came strongly or weakly across the head from ear to ear; none came (in the manner already described for the forks 128 and 256) from an oblique direction.



The other of us (W.) occasionally observed that while the 'full-right' or 'full-left' tones seemed "in the ear" and the 'middle' tones seemed "in the front," the 'half-right' or 'half-left' tones appeared to be at some distance.

We both noticed that the tones seemed much louder and of a 'fuller' quality when they were judged 'middle' than when they sounded 'right' or 'left.'

A third observer, who gave us only one sitting, was more sensible to apparent changes in volume of the tone than we were. Thus, when facing the fork 384, his answer was "median plane, rather to right" for the scale reading 60, and "the same but less volume" for the scale reading 70. Similarly, at 130 he answered "distinctly right," and at 140 "distinctly right but greater volume." Like us, he found the 'middle' tones more "bulky" than the 'lateral,' but he localised them in the occiput, "dorsally" as he termed it. He also noticed a difference in this respect between tones of different pitch. The fork 512 "had not the same spatial qualities as the lower notes"; it was "thinner, less viscous, more in the neck."

The same individual differences in the "subjective field" of hearing, as it has been termed, and the same differences dependent on the pitch of the fork, were observed when the observer's back was turned to the fork. They are all comparable with those previously described by Urbantschitsch[1]. Indeed the apparatus of this worker seems to have been somewhat similar to ours; but he always sounded the fork in a position symmetrical for the two ears, and consequently missed the various lateral effects which proved such a striking feature in our own experiments.

It seemed of interest to try the effect of presenting two forks of different pitch simultaneously at a given position of the T-piece. We found that when the forks were thus presented, each acted very much in the same way as it would have acted had it been presented alone. Thus at the scale reading 80 the fork 256, given alone, was judged left, and the fork 320, given alone, was judged right. When the two forks were sounded together at the scale reading the following answer was returned,—"low tone in left ear, both in right ear."

As a rule, the judgment of direction was not influenced by the loudness of the tone. We made some experiments expressly to test this point. On one or two occasions, however, the tone did go over to the opposite ear, when its loudness was increased. This will be more fully dealt with in the following section.

4. We have so far written of our experiments as if a laterally localised tone was heard in one ear and nothing was meanwhile heard in the other, despite the fact that it was being similarly and simultaneously stimulated. In the majority of cases this was so, but occasionally, while localising the tone in one ear, we could at the same time detect something at the other. This something we could only describe as a buzz. Save on one occasion (when it appeared to be a high overtone of the fork) the sound had no musical character whatever.

---

[1] *Arch. f. d. ges. Physiol.* 1881, Bd. xxiv. S. 579–585.

We cannot offer a satisfactory account of its origin. Perhaps the following observation only serves to increase the difficulty of an explanation. At the scale reading 120, the fork 512 gave "left with buzz in right." But when a resonator was placed between the fork and the T-piece, the sound went over the right ear.

There can be no doubt that owing to the presence of this buzz the observer experienced no little difficulty in localising the tone. Thus the fork 256, presented several times at the scale reading 90 during the same sitting, provoked the following answers,—"doubtful,—funny noise on left," "right," "middle," "half-left," "left," "right," "left," "half-left," "left," "right."

The occurrence of the buzz may be connected with the fact that on one or two occasions it was possible to make the sound appear at either ear by an effort of attention. This happened, for example, with the fork 320 sounding at the scale reading 160. At one sitting the following answers were returned for this position of the fork:—"middle or half-right," "doubtful, can put it in either ear by attention." At another sitting (with the same observer, the same fork and scale reading) the answers were "middle or half-left, buzz in right," "doubtful." At a third sitting the answer was "middle," and at a fourth sitting "middle, buzz on right."

In the following table we have collected from our note book the scale readings at which this buzz was at different times noticed:

Fork 128......80, 90, 160.
„ 256......55, 90, 120, 120.
„ 320......50, 60, 60, 60, 90, 110, 110, 110, 130, 160, 160.
„ 384......70, 95, 100, 130.
„ 512......50, 120.

These figures suggest the existence of some relation between the wave length of the tone and the buzz. The difference between 90 and 160 is approximately one quarter (67 cm.) the wave length of the tone 128. Half of this length (33·5 cm.) is approximately the distance between 55 and 90, and between 90 and 120, for the fork 256. And so on for the other forks. The buzz effect appears not to be confined to a single point of the scale but to spread over a fairly broad section of it. Occasionally, as for fork 512, intermediate positions at which we should have expected the buzz are not recorded. They may easily have eluded the observer's attention, as it was not always specially directed to detect the buzz.

5. The occurrence of this buzz may be partly responsible for certain differences in localisation which existed between us. They were most obvious in the case of the fork 320. Figs. 4 and 5 indicate how greatly we differed from one another in our judgment of direction. They also show a considerable disagreement between the observed and the theoretical (dotted) curves.

This disagreement from theory was still more marked in the case of the fork 256. Indeed in certain curves (cf. Fig. 6) the lateral effect was precisely the reverse of that expected. The curves obtained from the fork 250 also failed (but to a less extent) to agree with the theoretical curve. The forks 240 and 384, however, and the other forks, lower and higher than these, which were tried, gave satisfactory results.

We spent much time searching for the cause of these anomalies. We endeavoured (page 375) to improve the symmetry of the two arms of the tube. We also tried shortening each arm (by 56·7 cm.), and observed the effects produced by these altered conditions. But the curves obtained still differed widely from those theoretically anticipated.

Finally we proceeded to compare the amplitudes of vibration on the two sides of the apparatus. Accordingly we removed the tubes from the ear caps and connected each with the thin rubber diaphragm of a manometric flame. We observed these flames, in a revolving mirror, as usual, and it was immediately obvious that at certain positions of the T-piece there were considerable differences between the amplitudes of the two flames for the fork 256. The positions of the T-piece, which produced these differences in amplitude, corresponded to the positions where it had been observed that the anomalous lateral effects were most marked. That is to say, the deviations of the experimentally obtained curves from the curves theoretically expected were due to the tone on one or other arm of the tube being reinforced by resonance. Such gross differences in the intensity of the tone at each ear more or less completely masked the effects of phase differences. One of us (M.) was able with fair confidence to detect these intensity differences by listening with one ear applied first to one ear cap, and then to the other.

In the case of the forks 384 and 512, the two amplitudes, when compared by means of manometric flames, in the manner just described, were not appreciably different, whatever the position of the T-piece. It will be remembered that, in the curves given by these forks, there was no serious discrepancy between observation and theory. In the case of the fork 128, the manometric flames showed that at about the scale reading 60, one arm of the tube delivered a distinctly louder tone than the other arm. In accordance with this, we had observed a tendency for the fork 128, at the same position of the T-piece, to give a lateral effect which deviated somewhat from what would have been expected, if phase difference had been the sole influence determining localisation.

6. Some of our experiments were conducted with the express object of determining to what extent the localisation of a tone, at any given position of the T-piece, is determined by the immediately previous localisations at other positions of the T-piece. The following are examples selected from our note book, showing that, in one of us (W.) at least, there is a strong tendency to preserve the direction which had been observed in the previous position of the T-piece. The numbers refer to the positions of the T-piece:

40, *L*. 45, *L*. 50, *M*. 60, *R*. 55, *R*. 50, $\frac{R}{2}$. 45, *M*. 40, *L*.

Here 45 and 50, occurring after 40, *L*, are judged *L* and *M* respectively, whereas, when they occur after 55, *R*, they are judged *M* and $\frac{R}{2}$ respectively. Another example, one of 'complete ascent and descent,' may be given to show the same influence, the arrows showing the order of the observations:

	40	80	90	100	110	120	160	
→	*R*	*L*	*R*	$\frac{R}{2}$	*L*	*R*	*R*	
	$\frac{R}{2}$	*L*	$\frac{R}{2}$	$\frac{R}{2}$	$\frac{R}{2}$	*R*	*R*	←
→	*M*	*L*	*L*	$\frac{R}{2}$	*M*	*R*	*R*	

Each of these instances reveals the play of what has been termed the 'inertia' of judgment, a tendency to continue giving the same answers as before.

7. In certain experiments we endeavoured to reduce the loudness of the tone reaching one of the ears, by plugging one arm of the tube close to its insertion into one of the ear caps.

The following data were obtained by this procedure. The arrow heads indicate the order (descending or ascending) in which the scale readings given in the first column were presented. The answers, recorded in the second column, show that before the plug was applied, 'right' effects were obtained at 120 and 125, 'left' effects at 115 and 135, and 'middle' effects at 130. When the arm of the tube going to the left ear was plugged, so that the loudness of the tone reaching that ear was very considerably reduced, all the answers immediately became 'right.' But after a very short time (about a minute and a half), 130 began to be called 'middle' and 135 'left' or 'half-left.' After recording observations for about five minutes with the left arm of the tube thus blocked, the plug was removed, whereupon the answers were preponderatingly 'left' for all positions save 130. Finally, after about five minutes' practice, 120 and 125 began to produce more or less distinctly 'right' effects. We see, then, how rapidly we become adapted to binaural differences of intensity, how susceptible we still are to phase differences after such adaptation has been established, and how such adaptation, once acquired, persists for a short time after the sound intensity at the two ears has been equalised.

Scale reading	Without plug	Arm going to left ear plugged					Plug removed				
120	*R*	*R*	$\frac{R}{2}$	*R*	*R*	$\frac{R}{4}$	*L*	$\frac{R}{2}$	*L*	*L*	*L*
125	$\frac{R}{2}$	*R*	*R*	$\frac{L}{2}$	*R*	*R*	*M*	$\frac{R}{4}$	*M*	$\frac{R}{2}$	*R*
130	*M*	*R*	*R*	*M*	*R*	*R*	*M*	$\frac{L}{2}$	*M*	*M*	*M*
135	*L*	*R*	*R*	*L*	*R*	$\frac{L}{2}$	*L*	$\frac{L}{4}$	*L*	*L*	*L*
130	*M*	*R*	*M*	*R*	$\frac{L}{2}$	*R*	*M*	*M*	*L*	*M*	*M*
125	$\frac{R}{2}$	*R*	*R*	*M*	*R*	*R*	*L*	*L*	*M*	$\frac{R}{4}$	*R*
120	*R*	*R*	*R*	*R*	*R*	*R*	*L*	$\frac{R}{2}$	$\frac{R}{2}$	*M*	*L*
115	*L*	*R*	*R*	*M*	*R*	*R*	*L*	*L*	*L*	*L*	*L*
135	*L*	*R*	*R*	$\frac{L}{2}$	$\frac{L}{2}$	$\frac{L}{2}$	$\frac{L}{2}$	*L*	*M*	*L*	*M*
120	*R*	*R*	*R*	*R*	*R*	*R*	$\frac{R}{2}$	*L*	*L*	$\frac{R}{2}$	$\frac{R}{2}$
		↓	↑	↓	↑	↓	↓	↑	↓	↑	↓

8. It is possible that the markedly different effects produced upon us by the fork 320 (Figs. 4 and 5) were in part the outcome of individual differences in the liability to such adaptation. There were, however, other causes at work, responsible for the want of agreement between our judgments. For example, at any given position of the T-piece, one of us (M.) tended to place the sound rightward more often than the other (W.). When the scale reading was 100, that is to say, when the vibrations reaching each ear were as nearly as possible identical alike in

amplitude and phase, the answers obtained at different sittings were as follows:

	'Left'			'Middle'	'Right'		
	'full'	'half'	'quarter'		'full'	'half'	'quarter'
M. (39 answers)	0	1	0	3	14	18	3
W. (37 answers)	6	1	0	6	17	5	2

	'Left'	'Middle'	'Right'
M. (percentage answers)	2·6	7·7	89·7
W. (,, ,,)	19	16	65

The same tendency was also shown in some preliminary experiments conducted when the tube going to the right ear accidentally measured 2 cm. longer than that going to the left. Upon the discovery of this fault it was immediately rectified, but from the figures in the above table it would seem that, when this correction was made, an error in the opposite direction arose. That is to say, the tone became somewhat louder in the right ear than in the left and consequently there was a general tendency for tones which should have been judged medial to be localised on the right. But this does not affect the peculiar feature, to which we are drawing attention here, namely that the tendency to rightward localisation is considerably more marked in the one of us than in the other. Two alternative explanations of this difference between us suggest themselves. It is possible, in the first place, that one of us hears better in one ear than in the other. In the second place, it is possible that one of us tends, more than the other, to locate tones of doubtful direction on one side rather than on the other side of his body. That the latter explanation is the more likely is indicated by the results of several observations made at the scale reading 100 when the observers were sitting with their backs to the screen and fork. In all the 73 judgments above analysed, the observers sat with their faces to the screen and fork.

	'Left'		'Middle'	'Right'	
	'full'	'half'		'full'	'half'
M. (18 answers)	0	2	3	7	6
W. (10 answers)	3	0	5	0	2

In spite of the reversed position of each observer (the position of his right and left ears now being interchanged), the same greater tendency is manifest upon the part of M., as compared with W., to give 'doubtful' or 'middle' tones a right localisation. We may perhaps call this tendency to localise tones predominantly as right or left by the name 'lateral error.'

9. We are now able to realise the complexity of the influences deter-

mining the localisation of the tone at a given scale reading. We have drawn attention to individual differences (*a*) in the subjective field of hearing, (*b*) in the lateral error. We have shown that the ease of localisation is affected by (*c*) the production of a buzz in the opposite ear, (*d*) the effects of resonance, (*e*) the inertia of judgment, and (*f*) adaptation. All these factors may play a part in bringing about such individual differences of judgment as we met with in the course of our experiments.

The most striking of these differences are set forth in the following table. The various sittings are separated by short vertical lines:

Fork	Observer	Scale reading 40	Scale reading 70	Scale reading 120	Scale reading 150
320	M.	$R\ R \mid R \mid \frac{R}{2} \mid \frac{R}{4} R$	$L\ L \mid \frac{L}{2} \mid \frac{L}{2} \mid L \mid \frac{L}{4}$	$L\ L \mid \frac{L}{4} \mid \frac{L}{2} L \mid M\ M\ L \mid L \mid \frac{L}{2}$	$\frac{R}{2}\ \frac{R}{2} \mid M\ M \frac{L}{2} \mid R \mid M \mid \frac{R}{2} \mid$
	W.	$L \mid M \frac{L}{4}$	$R \mid R\ R$	$R \mid R\ R$	$L\ L \mid \frac{L}{4} M \frac{R}{4} L$
		Scale reading 50	Scale reading 70	Scale reading 120	Scale reading 160
884	M.	$R \mid R\ R \mid \frac{R}{4} \mid \frac{R}{2}$	$\frac{L}{2} \mid L\ L \mid \frac{L}{2} \mid M \frac{L}{2}$	$L\ L\ L \mid L\ L$	$\frac{L}{2} \mid L\ L \mid \frac{R}{2} L \mid \frac{L}{4}$
	W.	$M \mid L \frac{L}{2} M$	$R \mid R\ R$	$R\ R\ R \mid R\ R$	$M\ R\ R\ R \mid R$

It must be borne in mind that these differences occurred comparatively seldom, considering the very great number of our observations; and that with these exceptions, and those which we have previously mentioned, our judgments were just such as would have been expected, in accordance with the formula (page 368) $\phi = \sin 4\pi x/\lambda$.

B. *Theoretical.*

10. The results of our experiments confirm those arrived at by Lord Rayleigh[1], namely that the localisation of a tone of low pitch is dependent on the difference in the phase of vibration in which the tone reaches the two ears.

Now it is possible to regard the localisation of such a tone as due to a direct recognition of the phase difference at the two ears. Then we must suppose that the impulses, ascending the two auditory nerves, accurately preserve the characters of the stimuli which these nerves receive. We must suppose that each crest, each return to a position of equilibrium, and each trough of every sound wave effect corresponding changes in the nerve impulse, and that these several changes, occurring at different moments in the right and left auditory nerves, are trans-

[1] *Loc. cit.* pp. 224, 225.

mitted to the brain, where the impulses from each side are compared and where the decision takes place as to which nerve is carrying the vibration, the phase of which is in advance.

If this view be correct, it involves a very serious change in our conception of the origin and nature of the nervous impulse. We have hitherto believed that, under ordinary circumstances, all sensory stimuli (save, possibly, painful ones) act on the nerve fibres solely through the intermediary of end organs; and that these end organs may be excited, not only by the stimuli to which they are specially adapted to respond, but also by others, for example by electrical and chemical stimuli and especially by grosser mechanical stimuli, blows, for instance. Inasmuch as the sensations are similar despite the diverse character of such stimuli, we have hitherto believed that the impulses ascending a sensory nerve depend on the mode of response of the end organ and not directly on the character of the stimulus. Whether the stimulus to an end organ be uninterrupted, as in the case of a blow, whether it be intermittent, as in the case of a sound or a light stimulus, whether that intermission be slow or fast, gross or minute, as in the case of waves of auditory, chemical or electrical origin,—we have hitherto believed that, so long as the stimulus is 'effective' (that is to say, so long as it is able to excite the end organ), the sensory nerve merely reflects what is going on in the end organ.

It has been generally believed that nervous impulses are essentially similar and *sui generis*, and that they merely serve as a connecting link between the end organ at the periphery and the sensory centre. In the case of the visual apparatus, it is true that one and the same end organ is generally supposed to be capable of reacting in three or more different ways, the number of ways depending on the number of fundamental colour sensations in the theory of colour vision which is adopted; and similar views may conceivably be held for the sensations of taste and smell. No doubt, if we hold these views, we must admit that the nervous impulse, transmitted by one and the same visual (or other) end organ, is susceptible of corresponding variation in as many ways. That, however, is very different from supposing that these ways, in which the impulse along a single optic (or other) nerve fibre may vary, accurately reflect the different lengths of the light wave (or the different intramolecular vibrations of the substance tasted or smelled) on which they depend.

In the case of vibrations of sound,—despite the fact that they are much slower than vibrations of light,—it is nevertheless just as difficult to suppose that such characters of the stimulus are actually communicable to the auditory nervous impulse. It is very hard to believe that every crest and every trough of each sound wave produce an exactly corresponding crest and trough in the impulses transmitted along each auditory nerve. Were this so, we could no longer regard the sensory nerve as an intermediary, knowing no more of the exact nature of the external stimulus than the telegraph wire knows of the mental processes of the operator who transmits the telegram. We could no longer regard the sensory nerve impulse as being determined solely by the method of response of the end organ with which it is connected.

11. We hope to show that such a radical change in our views is unnecessary. We believe that both our own experiments and Lord Rayleigh's are capable of a very different explanation. We contend that, while binaural differences in phase are a *primary* cause of the observed lateral effects, these effects are *ultimately* referable to binaural differences in intensity. In the following demonstration of our contention, the only assumptions that we shall make will be (*a*) that the sound entering one ear is transmitted through the bones of the head to the internal ear of the opposite side, (*b*) that the retardation in phase, due to this ear-to-ear conduction, is small, and (*c*) that the two (direct and transmitted) sets of waves, at their meeting in one or other ear, arrive from opposite directions. We shall discuss the validity of each of these assumptions presently.

We may represent geometrically the motion of the air, when a tone is transmitted through it, in the following way. Suppose a point A (Fig. 7) to move with uniform velocity round a circle with centre O, and let ON be a fixed line. If P be the foot of the perpendicular from A on to ON, then as A moves round the circle, P will oscillate up and down the diameter of the circle. If A revolve round the circle n times per second then the motion of P will be that of an air particle in a train of sound waves of frequency n and amplitude OA, and the angle AON will represent the phase of the vibration.

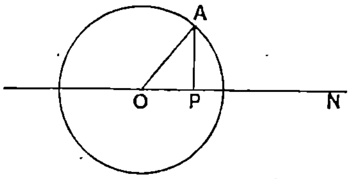


FIG. 7.

Suppose that the sound entering the right ear is of equal intensity to that entering the left ear but let the phase of the vibration at the right ear be ahead of that at the left. This is represented in Figs. 8, 9 where OA represents the amplitude at the right ear and $O'A' = OA$ that at the left ear and the angle AON is greater than the angle $A'O'N'$.

Draw OB parallel to $O'A'$, but shorter and in the reverse direction. This represents the amplitude of the vibration simultaneously transmitted by bone conduction from the left ear to the right in accordance with the first of our three assumptions. It is drawn shorter, because its amplitude is somewhat reduced in its passage across the head. It is drawn in a direction opposite to $O'A'$, in accordance with our third assumption. But we have to remember that during its passage through the head, the vibration suffers a slight retardation in phase. Accordingly draw OC equal to OB, the angle BOC corresponding to degree of this retardation. Then the right inner ear is at this moment receiving the two vibrations represented by the lines OA, OC, of which OA reaches the ear directly, and OC indirectly by bone conduction from the opposite ear. Complete the parallelogram $AOCR$. Then the diagonal OR represents the amplitude of the resultant vibration reaching the right ear at this moment.

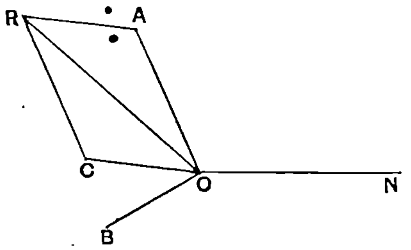


FIG. 8.

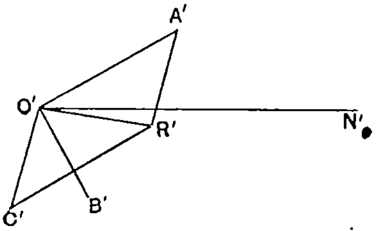


FIG. 9.

Similarly draw $O'B'$ parallel to but shorter than OA, and make the angle $C'O'B'$ equal to the angle COB. Complete the parallelogram $A'O'C'R'$. Then $O'R'$ represents the amplitude of the resultant vibration reaching the left ear at the moment when OR represents that reaching the right ear. But provided, according to our second assumption, that the angles BOC, $B'O'C'$ are not too great, $O'R'$ is clearly shorter than OR. That is to say, the right ear at this moment receives the stronger stimulus, and the tone is accordingly localised in the right ear.

If the difference of phase be increased until it is equal to two right angles, so that OA and $O'A'$ are in opposite directions, then it is easy to see that OR and $O'R'$ must be equal, and the tone is hence localised medially. If the difference of phase be still further increased, OR will be found to become shorter than $O'R'$; that is to say, the sound is now localised in the left ear. Indeed the direction of the sound is reversed, whenever the angle between OA and $O'A'$ (*i.e.* the phase difference) is increased by two right angles.

We have thus been able to prove that the differences in phase of the sounds, reaching the two ears, produce binaural differences of intensity; and that the observed changes of localisation are just what would be expected from these differences of intensity.

12. We have still to examine the three assumptions on which we have based this explanation, namely (*a*) that the sound entering one ear is transmitted through the bones of the head to the opposite inner ear, (*b*) that the retardation in phase due to such interaural conduction is small, and (*c*) that the two sets of waves, thus received at each ear directly and by bone conduction, meet coming from opposite directions. Let us now examine the validity of these assumptions.

It has become generally recognised of late years that it is impossible to stimulate the end organs of one ear without at the same time stimulating those of the other ear. When a sound is led to one ear, not only does it pass to the cochlea of that ear but, traversing the bony vault and base of the skull, it also travels to the opposite internal ear. The sound is localised by the observer on that side which is most strongly stimulated. His other ear, although it is stimulated, is 'physiologically deaf.' If the intensity of stimulation happen to be the same for the two ears, the sound is localised somewhere in the medial plane.

One is apt to neglect this bone conduction of sound from ear to ear, under conditions when two tones are led by tubes from separate rooms, one tone to each ear of the observer who sits in an intermediate room, that is to say, when every care is taken to prevent the two tones from coming together. Yet there cannot be the slightest doubt, even under these circumstances, as to the transference of each tone by bone conduction to the opposite ear. Mader[1] adduced presumptive evidence of such conduction by applying a small microphone to the roof of a skull. Two tones of nearly identical pitch were separately led, one to each ear hole of the skull. The microphone gave evidence of beats, showing that the tones were actually passing across the roof of the skull from one ear to the other[2].

---

[1] *Sitzungsber. d. kais. Akad. d. Wissens.*, Wien, 1900, Bd. CIX. Abth. 3, S. 37–75.

[2] Unfortunately one of the two tones used by Mader was generated in the room in which the observation took place. No doubt in consequence of this, the beats which were heard by aid of the microphone did not *entirely* disappear when one of the taps, with which the two tubes conducting the tones were provided, was shut off. However, this technical error does not sensibly diminish the value of the experiment.

Moreover, we ourselves, by leading the tone of a tuning-fork to one ear of an individual, proved that an appreciable amount of sound could be transmitted through his head to the other ear and could thence be conducted by a tube so as to be audible to the ear of a second observer.

There is evidence which goes to show that we are extraordinarily sensitive to the binaural differences of intensity thus produced. The well-known experiment of Weber is a case in point. If a vibrating tuning-fork be placed on the vertex of the skull, its tone will be localised somewhere midway between the two ears. But if a finger be introduced into one auditory meatus, the apparent position of the tone is at once changed. It is localised on the same side as the auditory meatus into which the finger has been introduced. This experiment is generally explained in the following manner. The insertion of the finger causes the intensity of the stimulus reaching that ear to be increased; it becomes greater than that reaching the opposite ear, and consequently the tone is localised laterally[1].

The following modification of this experiment, suggested by Schäfer[2], also affords evidence of the sensibility of the binaural mechanism to minute bilateral differences of sound intensity. A fork, fixed at some distance from one side of the observer, is very gently struck. The observer listens, and notes when the dying tone has become quite inaudible. He then inserts an appropriately attuned resonator into the ear which is nearest to the fork; whereupon, of course, the tone is at once softly heard again on that side, as if it came from the resonator. If the meatus of that more distant ear be now closed, the tone becomes at once stronger and its localisation approaches the median plane. If the meatus be then re-opened, the tone immediately leaps back again to the ear in which the resonator is inserted; and these alternations of position, according as the more distant ear is closed or open, can be repeated while the tone of the fork gradually dies away.

The importance of the transmission of stimuli by bone conduction from ear to ear is well seen in an experiment described by Thompson[3], in which two tones were generated in different rooms and were led by tubes, one to each ear of the observer. These tones were produced from two tuning-forks, having a pitch of 246 and 256 vibrations per second, respectively. Under these conditions, as is well known, beats are audible, just as if the two tones were presented to a single ear. Thompson concluded that under the conditions of binaural hearing above described, the tone-stimulus is transmitted along each auditory nerve to some common cerebral centre and that at this centre the beats arise. But this and the following interesting fact, also observed by Thompson, can be explained without recourse to such an unwelcome hypothesis, if we suppose that each tone is transmitted by bone conduction to the opposite ear and that the beats heard are due to the play of the two series of vibrations of different frequency on one and the same sense organ. Thompson found that if the tuning-forks (separated, as we have already described) were struck so faintly as to be inaudible when one or other was separately listened to by either ear, their beats were still perceptible when they were sounding together. Now there can be little doubt that two tones of approximately the same pitch act (*inter alia*) on one and the same end organ. Whereas the tone, received by direct transmission, may be too weak to stimulate an auditory end organ, nevertheless when a tone, likewise of subliminal strength, reaches that end organ, being transmitted by bone conduction from the opposite ear, the summation-effect of both these tones playing simultaneously on the same end organ may produce such re-inforcement that the threshold is overstepped and the end organ responds.

---

[1] Mach has suggested that when the fork is placed on the vertex, its tone passes by bone conduction to the inner ear of each side, and that the vibrations escape (in a direction contrary to that occurring under ordinary conditions) through the external auditory meatus to the outer air. Now when the finger is introduced into one meatus, the vibrations are prevented from escaping on that side. They are reflected back to the internal ear. Consequently that ear now receives a stronger stimulus than the opposite one, and the tone appears to have a lateral instead of a medial position.

[2] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* 1891, Bd. II. S. 111–114.

[3] *Phil. Mag.* 1878, Ser. 5, Vol. VI. pp. 383–391.

We feel that we are amply justified in considering at such length these physiological conditions of hearing, when they are apt to be overlooked even by the most distinguised writers on the physical aspects of the subject. We believe it to be of the utmost importance to remember that *we cannot stimulate one ear without at the same time exciting the other*, owing to bone conduction of the stimulus from ear to ear.

13. We proceed to justify our second assumption, which was that the retardation in phase, occurring during the transmission of the sound from ear to ear through the head, is small. Now, in the first place, the velocity of sound through bone,—especially through compact bone (and as we shall presently point out, this is probably the most important medium for the ear-to-ear conduction of sound),—is in all likelihood extremely high. And in the second place, the sound waves with which we are dealing are relatively long, the sound wave of the fork 256 measuring nearly 1·33 metres. Thirdly, the distance between the two

ears averages about 16 cm. There is therefore every reason for supposing that in the case of low tones the retardation in phase, due to the passage of the tone across the head, is relatively slight. In the case of the short waves of high tones the phase retardation may conceivably be so great as to cause reversal of the lateral effect, the angle *COB* having in this case a value between 180° and 360°. But with these higher tones, as Lord Rayleigh and we observed, the lateral effects cannot be obtained. Indeed it was the difficulty of explaining the localisation of *low* tones under ordinary conditions of hearing that led Lord Rayleigh to investigate the influence of binaural phase differences on localisation.

14. We have lastly to examine the validity of our third assumption, that, when the two sets of waves are received by one or other ear (the one set transmitted directly, the other by bone conduction from the opposite side), they meet coming from opposite directions. On this assumption was based our drawing of the lines *OB*, *O'B'* (Figs. 8, 9) in a direction opposite to their respective parallels *O'A'*, *OA*. The validity of this procedure will hardly be disputed. For no physiologist will maintain that the vibrations which are thus conducted, say, from the left ear across the head, reach the end organs of the right ear by passing to the temporal, parietal and occipital bones which cover the right side of the skull, and that they then turn in towards the right inner ear by way presumably of the drum and ossicles, now accompanying the vibrations which travel to the right ear directly. There can be little doubt that the direct route is the only one which it is worth while to consider; the vibrations passing from one ear at once to the petrous portion of the opposite temporal bone in which the auditory end organs of that side are lodged.

This view appears to be justified by Mader's experiments[1]. Applying his microphone to various points of the (dead) skull while a tone is led to one of its ear holes, he finds that the audibility of the tone heard by aid of the microphone varies at different points, and that this is dependent not on the distance of the point from the ear hole receiving the sound, but on the structure of the bone to which the style of the microphone is at the moment applied. The more compact the bone, the more easily heard is the sound.

Mader points out that whereas the cranial surface of the temporal bone contains spongy bone, the diploë, the petrous portion in which the auditory end organs are lodged, is one of the most compact in the skeleton, owing to the peculiar arrangement of the lamellae.

---

[1] *Loc. cit.* S. 61–68.

Mader also shows that, when the microphone is applied to different bony parts of the opened skull, the period, during which the dying sound of a tuning-fork, led to one ear of the skull, can be heard through the microphone, is very clearly dependent on the structure of the bone. From his experiments there can be no reasonable doubt that the vibrations, transmitted by bone conduction, reach the inner ear in the direction which offers the least resistance to their transmission; that is to say, they approach the opposite inner ear in a direction opposite to that of the incoming waves directly received from the exterior.

15. We have now satisfactorily established our contention that the effects of binaural phase differences, with which we have met in our experiments, are describable in terms of binaural intensity differences. It only remains to point out that similar effects, occurring under somewhat different conditions, can be explained on the same lines.

For example, when two forks of nearly identical pitch are led each to a separate ear,—or, indeed, if they be merely held one before each ear,—the observer will hear the beating sound passing alternately from one side of the head to the other, as described by Lord Rayleigh[1], provided that the pitch of the forks be not too high. Now we have ample experimental evidence to show that beats are heard in that ear which receives the stronger tone stimulus[2]. Consequently we have only to show that the loudness of the resulting stimulus periodically varies, now being greater in one ear, now in the other. This must inevitably be the case, if we admit the previously described effects of the transmission of each tone by bone conduction to the opposite ear.

For at one moment the phase of the tone directly reaching one ear is in advance of the phase of the tone that directly reaches the other ear. Consequently, as previously explained (pages 378, 379), the sound is localised in that ear in which the phase is more advanced. But the relative phases of the two tones are always changing, inasmuch as the tones are of slightly different pitch. The phase of the fork of higher pitch is continually gaining on that of the fork of lower pitch. When the two vibrations agree in phase, the sound will appear to be in the middle. As the more rapid vibration gains on the other, the sound will go over to the side directly receiving the former, remaining there until the phases are in opposition, when the sound will again appear in the middle. After this moment, it will go over to the opposite side and will remain there until the phases agree once more.

---

[1] *Loc. cit.* pp. 220 *et seq.* See also Melati, *Phil. Stud.* 1901, Bd. xvii. S. 454, and Sanford's *Course of Experimental Psychology*, 1900, p. 82, exp. 99 *b*.

[2] Cf. K. L. Schäfer, *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* 1890, Bd. i. S. 81–98, 300–309.

This is the interpretation which we venture to think is applicable to Lord Rayleigh's experiments. And we think that it can be extended so as to explain the localisation of tones of low pitch under ordinary conditions of hearing. As Lord Rayleigh has pointed out, the length of the sound waves generated by such low tones is so long that the head is relatively too small for the intensity of the sounds directly reaching each ear to be perceptibly different. But we contend that the sound reaching the nearer ear passes by bone conduction to the opposite ear, and that during this passage its phase is slightly retarded. Arrived at this ear, it meets (coming from an opposite direction) the vibrations directly conducted, which are further retarded in phase owing to their passage through the external air. These directly conducted vibrations are in turn transmitted by bone conduction to the opposite, *i.e.* the nearer, ear. Now we have already demonstrated that under such conditions a tone is localised in that ear at which the phase is more advanced. Consequently the tone will be localised at the ear nearest to the source of sound,—unless the distance between the ears is greater than half its wave length.

16. We conclude, then, that it is unnecessary to suppose that the binaural mechanism is directly sensible of the differences of phase with which the sound vibrations arrive at the two ears. We have pointed out the difficulties attending this view, and we have shown that, in instances to which this theory has been applied, an explanation is possible in terms of binaural differences of intensity. To us this explanation seems far preferable to the other. The history of acoustics shows how repeatedly attempts have been made to prove that, when two tones are directly led to *one* ear, we are conscious of the relation in which the phases of these two sets of vibrations stand to one another. But not one of these attempts has been able to withstand close investigation. We contend that there is every reason to believe in a like insensitiveness towards phase differences, when the two tones are led to *separate* ears.

SOME DATA FOR A THEORY OF THE AUDITORY PERCEPTION OF DIRECTION.

BY A. M. HOCART AND WILLIAM McDOUGALL.

Summary of Contents.—The aim of the experiments is to throw light on the relative importance of the various conditions of stimulation of the ears operative in localisation of sound.—First series, localisation of noises and of pure tones compared.—Second series, localisation of noises, both ears having artificial meatus.—Third series, localisation of noises when meatus is blocked on both sides.—Fourth series, monaural localisation of noises and pure tones.—Fifth series, monaural localisation of noise of varying quality and with artificial meatus.—Some condition indicated as operative in addition to the two generally recognised conditions.—What is this further condition?—The localisation of pure tone of deep pitch.—Some difficulties in the way of any theory.

ANY theory of the auditory perception of the direction of a source of sound has to deal with two problems. First, what are the conditions of stimulation of the sense-organs that render such perception possible? Secondly, these conditions being given, how does the organism utilise them as the basis of its perception of direction?

The observations we have to report bear chiefly on the former problem. Importance has been attached by various authors to five conditions of stimulation, namely (1) difference of intensity of stimulation of the two ears[1]; (2) modification of the relative strengths of the elements of a complex vibration by the shell of the ear[2]; (3) difference of phase of the waves striking the two ears at any given moment[3];

---

[1] Three varieties of this view have been proposed:—A. Steinhauser (*Phil. Mag.* 1879) argued that the essential condition is the difference of intensity of the total stimulations of the two ears; Prof. E. Mach (*Archiv f. Ohrenheilkunde*, 1874) that it is the differences in the relative intensities of the partial tones reaching the two ears, the differences being produced by the shells of the ears, on which the waves fall at different angles; Lord Rayleigh (*Trans. Musc. Assoc.* 1876) took the same view as Mach, but attributed the differences in the relative strengths of the partial tones reaching the two ears to the refraction of the waves that reach the remoter ear. It is obvious that this view is applicable to the case of pure tones only in the first of these three forms. Among others who have defended it in this form is Prof. von Kries (*Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. I. and *Arch. f. Physiol.* 1877).

[2] S. P. Thompson (*Phil. Mag.* 1879 and 1882).

[3] Lord Rayleigh (*Phil. Mag.* 1907)

(4) stimulation of the skin of the meatus or of the tympanic membranes by the air-waves[1]; (5) differential stimulation of the sense-organs of the semicircular canals according to the direction of the source of sound[2].

It seems to be fully established by the observations of several workers that the first two of these conditions are commonly concerned in the perception of direction. The recent work of Lord Rayleigh seems to prove that in the case of deep pitched sounds the third condition is operative. But whether the fourth and fifth, or any other conditions, may also cooperate remains at present an open question[3].

It seemed that it might be possible to throw further light on the relative influences of these conditions by determining the threshold of auditory discrimination of difference of direction, when each (or several of them) was in turn excluded as far as possible.

First Series.

Our first experiments were directed to exclude the operation of the second of the conditions of stimulation mentioned above, namely the influence of the shells of the ears upon a complex sound. One obvious way of effecting this is to use as stimulus a pure tone and to compare the threshold of discrimination of difference of direction for noises with that for pure tones. This we did in five subjects, of whom two were ourselves. Since it would have been difficult, if not impossible, to make use of any of the various ingenious forms of sound-perimeter in stimulating with pure tones, we dispensed with all such mechanical aids and adopted a very simple procedure.

A circle of one metre radius was drawn in the centre of the floor of the laboratory[4], and the subject was seated on a chair in such a position that the centre of his head was as nearly as possible above the centre of the circle. The circle was marked in degrees clockwise, and the subject faced the position marked 0°, having the point 90° on his right hand, 270° on his left. The thresholds of discrimination were determined in all cases at positions 0°, 270° and 180°, which for convenience we call positions *A*, *B* and *C* respectively. In several cases the threshold was determined in other positions, especially at 90°; but this latter was found to coincide approximately with that at 270°.

---

[1] A. Weber (*Ber. d. kgl. sächs. Ges. der Wiss.* 1851).

[2] Chiefly by W. Preyer (*Pflüger's Archiv*, Bd. 40) and, in a modified sense, by Prof. Münsterberg (*Beiträge zur experimentellen Psychologie*, Heft 2, Freiburg i. B. 1889).

[3] The history of the problem and its position at the time of publication of his book, *Studies in Space Perception* (London, 1901), have been fully stated by Prof. A. H. Pierce. It is therefore unnecessary for us to repeat the statement here.

[4] We regret that the only room available was one not specially well adapted to such experiments. We were able to secure the necessary quiet by working only at selected times. We attempted to minimise the influence of irregularities of reflection from the walls by keeping the position of the subject constant, and some of our experiments, where the exclusion of this factor seemed specially important, were made on an open lawn. The room in which most of our experiments were made was placed at our disposal by Prof. Gotch; we wish to acknowledge our indebtedness to him for his kindness.

Our procedure involved the combination of the method of minimal changes with that of right and wrong cases that has been applied by one of us in previous psycho-physical researches[1]. As a complex sound-stimulus we used, in all cases where the contrary is not stated, the sound of two sharp turns of the head of a keyless watch[2] separated by an interval of about half a second. This we call 'the noise.'

The subject being seated with closed eyes the operator uttered a warning 'now,' and made 'the noise' twice at an interval of from one to three seconds, the watch being held always at some point in the plane of the subject's ears and directly above some point on the circumference of the circle drawn on the floor. The position of the watch was controlled by a plumb-line dependent from its head and just clearing the floor. The subject was required to say in every case whether the second of the two noises seemed to come from the same place as the former or from a different place, using the words 'same' and 'different[3].' In determining the threshold of discrimination at any position of the horizontal plane, say position *A*, the former of the two noises of each experiment was made always at the position *A*, and the second noise was made either in the same position, or at a point a given number of degrees to one side of this standard position. On beginning any series, the interval was always made considerably greater than that which we had reason to believe was approximately the measure of the threshold of discrimination; a short series was made in which the second noise was made always either at this interval from, or at the same position as, the first noise; the succession of the two positions of the second noise was varied as irregularly as possible, but the second noise was given in the two positions ('same' and 'different') with approximately equal frequency in the series made with each interval. When the subject had given ten correct answers without error, a new series was made in a similar way, with the second noise at a lesser distance from the first; and if, in this series of ten tests, no error was made, the interval was again diminished. When the subject made one wrong judgment in the series of five cases in which the position of the second noise was different, the series was prolonged to twenty tests; if in these further tests at this interval he made no wrong answer in the case of the five tests in which the position of the second noise was different, a new series was made with diminished interval. But if he made again one wrong judgment (*i.e.* altogether two wrong judgments in the ten cases in which the position of the second noise was different), this interval was accepted as the measure of his threshold of discrimination at the position *A*. A further series, in which the interval was again diminished, was then made to confirm the result, and in almost every case the proportion of wrong answers given was then considerably greater. The subject's replies, in the cases in which the position of the second noise was the same as that of the first noise, were not regarded in reckoning the proportion of right and wrong answers, but these tests served as a necessary check upon the subject's natural bias to give the answer 'different' whenever he was in doubt; and it was of course necessary that the majority of his answers to these tests should be correct. So long as the interval between the two positions was greater than 30°, the interval was diminished at each step by 10°; and from 30° downwards the intervals were diminished as follows:—

---

[1] *Brit. Journ. of Psychol.* Vol. I. p. 165.

[2] In this we followed Münsterberg (*loc. cit.*). No attempt was made to keep the intensity of the stimulus constant. Rather we made a point of varying the rate and duration of the turning movement as much as possible in order that the stimulus should not be constantly of the same intensity and quality. Some workers in this field (*e.g.* Starch) have used a stimulus of great constancy of quality and intensity; this introduces a datum which complicates the operative conditions of stimulation in a way undesirable for our purpose.

[3] We might have required the subject to say whether the second noise seemed to come from right or left of the position of the former noise, but in that case the subject might have learnt to discriminate between the rightward and leftward positions of the second sound in successive experiments rather than between the positions of the first and second sounds. Starch (*Psych. Rev.* Monograph Supplement, No. 28, 1905), who conducted a series of experiments in this way, found that his subjects did in fact tend to discriminate in this way.

30°—25°—20°—15°—10°—7°—5°—3°—2°.

We transcribe the judgments recorded in the determination of one threshold in order to make clear the procedure. Position *C*—intervals to the left of *C*.

$$40^\circ\frac{\angle\ \angle\ \angle\ \angle\ \angle}{\angle\ \angle\ \angle\ \angle\ \angle}\qquad 30^\circ\frac{\angle\ \angle\ \angle\ \angle\ \angle}{\angle\ \angle\ \angle\ \angle\ \angle}\qquad 25^\circ\frac{\angle\ \angle\ \angle\ \angle\times\angle\ \angle\ \angle\ \angle}{\angle\ \angle\ \angle\times\angle\ \angle\ \angle\ \angle\ \angle\ \angle}$$

$$20^\circ\frac{\angle\times\angle\ \angle\times\angle\ \angle\ \angle\ \angle\ \angle}{\angle\ \angle\ \angle\ \angle\times\angle\ \angle\ \angle\ \angle\ \angle}\qquad 15^\circ\frac{\angle\times\times\angle\ \angle\times\angle\ \angle\ \angle\ \angle}{\angle\ \angle\ \angle\ \angle\times\times\angle\ \angle\ \angle\ \angle}$$

$$10^\circ\frac{\angle\times\angle\times\angle\ \angle}{\times\angle\times\times\angle\times}.$$

Each ∠ implies a right answer, each × a wrong answer; those above each line are the answers given when first and second noises were made at the same position; those below the line are the answers given when the positions of first and second noises were at the angular interval denoted by the figure preceding the line.

In this case 15° was accepted as the measure of the threshold of discrimination under the given conditions.

When the whole series runs so smoothly as that above, the successive short series support and confirm one another, and the figure arrived at may be accepted with some confidence. But in some cases the series runs less smoothly, and then it becomes necessary to prolong, or to repeat, the short series at one or more of the steps; and whenever we felt dissatisfied with the series for any reason, it was so prolonged until we felt that the conclusion to be drawn from it was reliable; in some cases as many as forty, fifty, or more tests were made at one step of the series. We further refined the measure of the threshold by adding in certain cases + or − to the figure expressing it; *e.g.* if at one step (say, at interval 15°) no errors were made and at the next step (interval 10°) more than two judgments in ten were wrong, the threshold was represented by the figure 10° +; and usually in such cases the series was further prolonged.

The tests of the earlier steps of such a series serve to train the subject for the particular task and to render his judgments in the region of the threshold much more reliable than they would be in the absence of such practice[1]. In the case of experienced subjects these preliminary steps were generally shortened, if the subject expressed absolute confidence in his judgments. The subject was instructed to report at the conclusion of the experiments of each step of a series, whether his judgments were expressed with complete confidence in their correctness, with moderate confidence, or without confidence. This served as further confirmation of the accuracy of the determination; since in nearly all cases the subject expressed complete confidence in the judgments of the earlier steps of each series, and lack of confidence as the interval approximated to the threshold value.

The experiments with pure tones were conducted in exactly similar fashion, except that the place of the watch-winder as source of sound was taken by the small wooden resonance-box of a tuning fork of frequency 512. The position of the resonator was regulated by a plumb-line depending from it, and it was held by the hand of the operator with its open end directed towards the subject's head. The operator sounded the fork by tapping it on his knee so gently that its note was not audible to the subject until it was brought up to the mouth of the resonator. In each experiment he held it before the mouth of the resonator for about one second and then, after an interval of from one to three seconds, brought it a second time before the mouth of the resonator for about one second (the fork being struck a second time in the interval in some cases, but not invariably). As in the case of the noises, the subject was required to answer 'same' or 'different' in reference to the position of the second sound; and the position of the resonator at the second sounding was varied as in the noise series (*i.e.* the second tone was sounded irregularly but equally frequently at the 'same' and the 'different' positions). These experiments were, of course, much more troublesome than those with noises; for in the latter case slight noises made by the operator in moving the position of the sounding body did not invalidate the experiment; whereas any such noises, especially any noise made by the movements of the hands and arms of the operator, vitiated the experiments with pure tones. The subject found it easy to say whether he perceived any such noise, because if he did so his judgment of position was at once determined by it. He was instructed to notify all such instances and they were rejected. By dint of wearing only soft clothing, having the forearms bared, and moving with great care this difficulty was overcome.

---

[1] Immediately after expressing each judgment, the subject was told whether it was right or wrong.

The question arises—Can the tone heard by the subject in these experiments be regarded as a strictly pure tone? This is a physical problem on which we do not feel competent to form an opinion; but we think that the results of the experiments shew that the tone was sufficiently pure for the purposes of the experiment. For, so long as the fork was sounded so gently as to be inaudible save when held before the resonator, the threshold of discrimination of its position was very distinctly higher than that for noises; whereas if the fork was sounded more loudly in any series, the threshold was found to approximate to, or to be the same as, that for noises[1].

In this way we obtained the results set down in Table I.

In this table *N* stands for 'noise' and *P T* for 'pure tone.' The second position of the sound was to the (subject's) left of positions *A* and *C* and backward from position *B*. We would regard the figures expressing in degrees the thresholds of discrimination as most accurate in the cases of the last two subjects, ourselves namely; for each of these determinations was several times repeated upon different occasions, and the thresholds were also determined in other positions, *e.g.* the position corresponding to *B* on the subject's right side, *i.e.* opposite the right ear, and also to the right of positions *A* and *C* and forward as well as backward from *B*.

---

[1] It is, we think, quite clear that the sound of a tuning fork transmitted to the ear by a telephone (the form of stimulus used by several observers in experiments on localisation of sound) must be regarded as a very impure tone.

TABLE I.

Subject[1]	Position *A*		Position *B*		Position *C*	
	N	*P T*	*N*	*P T*	*N*	*P T*
P. A.	3° –	25° –	15° +	45° –	7° –	10° –
G. B. S.	3° –	20° +	10° +	70° (?)	7°	10°
A. H.	5° +	20°	20°	35°	17°	70° (?)
A. M. H.	3°	20°	15°	30° +	7°	30° +
W. McD.	3°	20° –	15° –	30° +	10° –	20° –

In the case of the stimulation by noise, the results obtained on different occasions were in all cases in close agreement. In the case of the experiments with pure tones there were certain discrepancies; *e.g* one of us, one day when feeling tired, could hardly make any correct judgments, even when the intervals were made very large; and the strain on the attention is always very considerable in the experiments with pure tones: again, on another day, we found surprisingly low thresholds, approximating closely to those for noises; but on this day we had used a larger fork; and there can be no doubt, we think, that its tone was less pure, so that even the use of the resonator did not suffice to render practically pure or simple the vibrations reaching the ears. The figures given represent our best performance in each case. We regard with suspicion the exceptionally large figure 70° appearing in columns 4 and 6. We are inclined to believe that the figure is unduly high in both cases, and that a lower figure would have been obtained if we had found opportunities of repeating the experiments under favourable conditions; but this unfortunately was not possible.

The conclusion to which we would first draw attention is that the threshold for noise is in all cases lowest at *A*, highest at *B*, and of intermediate value at *C*. This result is opposed to that of Prof. Münsterberg[2], who first determined these thresholds, and who found (using one subject only) that the threshold rises in magnitude from *A* to *C*, *i.e.*

---

[1] We wish to record here our obligation to the three gentlemen who took part with us in these experiments, namely Mr P. Annett, Mr G. B. Smith, and Prof. A. Hoernle.

[2] *Op. cit.*

from the position in the median plane in front of the subject all round the semicircle to the median position behind him. Our conclusion is in harmony with the results of Bloch[1] and of Starch[2].

The same relative values of the thresholds at the three positions result in the case of pure tones also, with the exception of the doubtful case of the subject A. H. and the case of subject A. M. H., for whom the thresholds at *B* and *C* are, as nearly as we could measure, of the same value.

The principal result of this series is the demonstration of the superior accuracy of localisation of noises and complex tones as compared with that of pure tones. The differences of the two thresholds are fairly consistent in the results found with all the five subjects. The difference is relatively greatest at position *A*; and the superiority of discrimination at position *A* over that at position *C*, which is very decided in the case of noise, is abolished or reversed in the case of pure tones. These results are consistent with the view that the superiority of discrimination of noises and complex tones is due to the influence of the shells of the ears upon the quality of the complex, and that the surprisingly great accuracy of localisation at position *A* is chiefly due to their influence.

It is to be noted that, since in all cases the objective intensity of the second stimulus bore no constant relation to that of the first, differences of intensity of the two sensations could not serve as a basis of the judgment as they may (and, according to the report of Starch, do) if the intensity of the stimuli is constant.

As regards introspective observation, it was usually extremely difficult to say anything about the basis of one's judgment, further than that usually the sound was simply and directly heard as there or there. Though the absolute localisation was frequently at fault, both standard and comparison stimulus seeming to be to one or other side of their true positions, the relative localisation was usually the basis of the judgment. Occasionally, especially in the case of the larger intervals and the pure tones at positions *A* and *C*, the subject had the impression that one ear was more strongly stimulated than the other; and this seemed to be in these cases the basis of the judgment.

We would draw attention to the fact mentioned above, namely that a very slight and unappreciated impurity of the tone suffices to reduce the threshold of discrimination to, or very nearly to, the same value as that of noises. This fact illustrates the extreme delicacy of the processes which result in the greater accuracy of localisation of noises and complex tones as compared with that of pure tones. We do not think that the difference of the threshold value found for the noises and the pure tones was due in any degree to lower intensity of the stimulation by the pure tones, for the tones were in all cases very distinctly audible; and, though it is possible that the tones gave on the average rather weaker stimulation than the noises, feeble stimuli seem to be localised as readily as stronger ones so long as their intensity is distinctly above the absolute threshold value.

---

[1] E. Bloch, 'Das binaurale Hören,' Wiesbaden, 1893.

[2] *Psychol. Rev.*, Monograph Supplement, No. 28, 1905.

Second Series.

We consider next experiments in which we attempted to rule out the influence of the shells of the ears in facilitating the localisation of complex sounds. We argued that, if difference of intensity of stimulation of the two ears and modification of the complex sound by the shells of the ears are the only conditions of stimulation operative in localisation or discrimination of position, then ruling out the influence of the shells of the ears should raise the threshold of discrimination of position of complex sounds to the same magnitude as that of pure tones. We covered the shells of the subject's ears with plasticine (modelling clay), building up on either side a flat cone around a pencil which was held in the meatus by the subject. On the pencil being withdrawn, there remained an artificial meatus opening at the apex of the smooth flat cone. Both ears of the subject being thus prepared, the thresholds for noise at positions *A*, *B*, and *C* were determined as in the previous series. The results are stated in Table II. The experiments are a little disagreeable and were made on ourselves only.

TABLE II[1].

	Position *A*	Position *B*	Position *C*
A. M. H.	7° (20°)	20° (30°+)	5° (30°+)
W. McD.	10°-(20°-)	20° (30°+)	10°-(20°-)

As was to be expected, the greater accuracy of discrimination at position *A* as compared with position *C* is abolished, but in both subjects the thresholds remain in all cases distinctly lower than those for pure tones. It may be suggested that this greater accuracy of discrimination of positions of noises under these conditions is due to the artificial meatus playing in an imperfect manner the part of the shell of the ear. But it seems improbable that, even if it does modify the complex sound waves differently according to the directions from which they come, this entirely strange system of delicate modifications could account for the considerable differences between these thresholds and those for pure tones. The results of these experiments therefore suggest the cooperation of some further condition or conditions in the localisation of complex sounds, in addition to the influence of the shells of the ears and the different intensities of stimulation of the two ears.

---

[1] The figures in brackets are the thresholds found for pure tones at the various positions when the ears were in their normal condition. They are introduced in this table for convenience of comparison.

Third Series.

Another way of excluding the influence of the shells of the ears is to block completely the meatus on both sides. We found that we could more effectively block the meatus by means of the forefinger firmly applied than by filling it with wool and wax and covering it with pads; we therefore adopted this simple method and determined the thresholds for noise, while both ears were so obstructed. The noise of the watch-winder was not loud enough to be heard under these conditions and we therefore used as stimulus the noise produced by striking with a small wooden mallet an iron bar held vertically between finger and thumb above any desired point of our circle. We obtained the following results:

TABLE III.

	Position *A*	Position *B*	Position *C*	
A. M. H.	5°	40°	15° +	noises
W. McD.	5°	20° –	20° –	

On attempting to determine the thresholds for pure tones under similar conditions, we found it necessary to use a larger fork and to sound it more loudly. We then obtained the startling result that the thresholds for both of us were lower than when both ears were open. On then determining the thresholds with this fork and with both ears open, we found that they also were abnormally low and in fact approached very closely to the thresholds for noise; thus in position *A* A.M.H. gave threshold 5° with both ears open and 5° with both ears closed while W.Mc.D. gave 5° – with both ears open and 5° with both closed. It was obvious, when the fork was loudly sounded, that its note was very impure, and there can be little doubt that these apparently anomalous results were due to the impurity of the tone; although in the course of the experiments the fork was sounded so softly that the tone was near the limit of audibility and in many cases had to be repeated because the subject failed to hear it.

The results of this third series seem to bear out the indication afforded by those of the second series, namely, that our greater accuracy in localisation of noises as compared with that of pure tones depends not only upon the influence of the shells of the ears, but also upon some further cooperating condition (or conditions) of stimulation. For it would seem that on the firm closure of the meatus the sound reaching the internal ear (presumably chiefly by conduction through the bones of the skull) cannot be modified by the shells of the ears in any way that could facilitate localisation; yet the thresholds are not, save in two cases out of the six, so high as those for pure tones, and in position *A* the threshold in both subjects was very decidedly lower than that for pure tones. Further, it seems hardly possible that, when both ears are stopped, the variations of the relative intensities of stimulation of the two ears can be so nicely graded as when the stimulus reaches the internal ear in the normal fashion; which consideration would lead us to expect higher thresholds for noises when both ears are stopped than for pure tones when both ears are open, if superior accuracy of localisation of noises is wholly due to the influence of the external ear.

Fourth Series.

In a fourth series of experiments we determined the thresholds of discrimination, when the factor of difference of intensity of stimulation of the two ears was precluded by the stopping of one meatus. We used again the sound of the watch-winder, and, in order to make sure that only the unstopped ear was stimulated, we tested repeatedly the sound produced by it, satisfying ourselves that it was inaudible when both ears were closed. The right ear being firmly stopped with the forefinger and the left ear being in its normal condition, the thresholds for the noise were determined as before; the results are stated in Table IV.

TABLE IV.

	Position *A*	Position *B*	Position *C*
A. M. H.	10°	15° (50°)	20° –
W. McD.	15° +	25° + (55)°	25° –

The figures in brackets express the thresholds found when the standard noise was made at the position opposite *B*, *i.e.* opposite the closed ear.

It will be noticed that the thresholds at *B* (*i.e.* opposite the open ear) are but little raised by the closing of the opposite ear, while those at *A* and *C* are considerably raised, though with one exception remaining lower than those for pure tones at the same positions. These thresholds seem to us surprisingly low; they seem to indicate that the cooperation of the two ears in the localisation of complex sounds is a condition of minor importance only, that its share in the total result is decidedly less than that of the influence of the shell of the ear together with that of any other operative conditions.

On attempting to discriminate the positions of pure tones under similar conditions (*i.e.* one ear stopped and the tone so feeble as to be inaudible if both ears were stopped) both observers found it impossible to make any true judgments; the tone seemed to have no definite local quality, and our guesses were wrong as frequently as they were right, even when the fork was sounded first on the one side and then on the other side of the head. Both subjects tried also to guess the direction of movement of the continuously sounding fork when it was moved from the right to the left side, or *vice versa*, and here too we failed completely. Lastly, the fork was sounded irregularly at the extremities of the four principal diameters of the circle (*i.e.* at eight positions separated by intervals of 45°) in the horizontal plane passing through the head, and the subject (one ear being stopped) attempted to name its position in each case. The guesses of both subjects were within 45° of the true positions only slightly more frequently than chance coincidence would account for.

These results are compatible with the view that difference of intensity of stimulation of the two ears is the only operative condition in localisation of pure tones.

Fifth Series.

Since the experiments of the foregoing series suggested that, in the localisation of complex sounds, the modifications of the complex waves by the shells of the ears and the differences of intensity of stimulation of the two ears are not the only operative conditions, our next step was to attempt to localise a source of sound, when both these factors were completely ruled out. This involved some very laborious experimenting. In order to preclude the former condition, we covered the shell of the left ear as before with a flat cone of wax perforated by a circular hole prolonging the meatus. We took the further precaution of varying in each test the character of the sound used as stimulus; for it seemed possible that, if a stimulus of constant character were used, the relative intensities of its partial tones might be modified by the artificial meatus or by refraction, according to the direction of its incidence, sufficiently to afford some basis for a judgment of direction. And in order to preclude the second condition, one meatus (that of the right ear) was firmly stopped, and each stimulus was tested and proved to be inaudible when both ears were stopped.

The subject was seated, facing south, on a chair in the middle of an open lawn. The eight principal points of the compass were marked on the lawn on the circumference of a large circle drawn round the subject. The stimulus was the sound made by the falling of a small pellet of wax, of a pea, or berry, or other small pellet, upon some one of a number of surfaces of various character (box-lids of wood, cardboard, or metal, plates and dishes of china, etc.); in this way a sufficient variety of quality of sound was secured to prevent the subject becoming familiar with the sound of any one quality. The sound was made always at one of the eight principal points of the compass and the subject was required to name the point. We worked only on still days, as quiet was essential for these experiments. Each experiment was made as follows: the subject's right ear being filled with wax and covered with a large soft pad tied to the head, he stopped his left ear (over which was the wax covering with artificial meatus) with his forefinger; his eyes were kept closed throughout the experiment. The operator then, moving so quietly that his movements were quite inaudible to the subject, took up his position at one of the eight marked points, provided with a pellet of some sort in one hand and a surface on which to drop it in the other. A third person then shouted 'ready' to the subject, who thereupon removed his finger from the artificial meatus and awaited the sound. The operator began by dropping his pellet from a point but little above the surface, so that the sound was inaudible to the subject, and repeated the stimulus again and again, increasing the height of the drop by one or two inches at each repetition, until the subject heard the sound and signalled the fact. Frequently the subject could make no judgment of direction on first hearing the sound; the sound was then repeated (again and again if necessary) until the subject expressed a judgment. If the position named by the subject was the true position of the source of sound, or if it was at an interval of only 45° from the true position, the stimulus was then tested in the following way, in order to discover whether it was strong enough to affect the closed and padded right ear. The artificial meatus of the left ear was closed by the subject's forefinger, and the pellet was dropped several times from the same height as that used in the experiment; if under these conditions the subject could by straining his attention detect the sound, the experiment was rejected. If he failed to detect the sound under these conditions, the operator dropped his pellet from successively higher positions, until the subject signalled his perception of it. The results of the significant experiments were divided into two classes, the doubtful and the satisfactory cases. The latter were those cases in which the sound did not become audible to the subject (with both ears closed), until the pellet was dropped from a point considerably higher than that from which it was dropped in the localising tests. The doubtful cases were those in which the subject detected the sound when the drop was only a little greater than that used in the experiment. Since the meatus of the right ear was closed much more effectually than that of the left ear (the latter being closed only by placing the finger-tip on the aperture of the artificial meatus) we think we may assume with confidence that in the satisfactory cases the right ear was not appreciably stimulated; and we think that it is very improbable that it was appreciably stimulated in the doubtful cases, which, therefore, we regard as not altogether devoid of significance.

The results are given in Table V.

TABLE V.

		No. of experiments	No. of right answers	No. of answers of error not greater than 45°
(1)	A. M. H.	82	17	51
		Chance would give	(10)	(30)
(2)	W. McD.	24	8	10
		Chance would give	(3)	(9)
(3)	W. McD.	118	35	82
		Chance would give	(15)	(45)

The third horizontal space of this table requires a few words of explanation. A number of experiments (94) were made with this subject in the way described above, with this difference only—the subject's left ear was in its normal condition; the additional precaution of covering the shell with wax to form an artificial prolongation of the meatus having been adopted only in the later (24) experiments with this subject. But the variation of the character of the sound with each experiment must, it would seem, render the modifications of its character by the shell of the ear of no value as a guide to localisation; therefore we have added the results of these 94 experiments to those of the 24 made under the more strict conditions and have stated them in the third space of the table.

All the doubtful cases are excluded from this table; if they were added, the proportion of right, and of approximately right, answers would have been considerably increased; and, as was said above, we think it improbable that the right ear was appreciably stimulated in these doubtful cases.

The excess of right answers and of approximately correct answers (*i.e.* of error not exceeding 45°) over the number that should be expected, if the subject's answers were mere guesses, seems to us large enough to prove that the answers were not mere guesses, but were the expression of judgments determined by some condition of the stimulation; and, since one ear only was stimulated and since the influence of modification of the sound waves by the external ear seems to have been excluded, the experiments seem to indicate that some factor other than these two plays a part in determining judgments of direction of the source of sound.

In estimating the significance of the excess of correct and approximately correct localisations over the number to be expected from chance alone, it must be remembered that the sounds were in all cases very little above the threshold of audibility, and that, even under the most favourable conditions, sounds of extremely low intensity are less well localised than those of higher intensity; further, that nearly all subjects are very liable to confuse sounds coming from positions directly in front and directly behind the head under the most favourable conditions, and many of our errors were of this nature.

The introspection of both subjects confirmed the indication afforded by the figures. In a few cases only was the sound heard distinctly as coming from the true position when it was first caught. Usually it seemed at first to have no definitely local quality. We found then that we could in many cases arrive at a judgment of its position by expecting it at each of the various positions in turn; when the sound came it seemed to disappoint or to fulfil the expectation. In the former case we tried again, expecting it in a different position; in the latter case (*i.e.* when the sound satisfied our expectation) the position to which expectant attention was directed was named. In what exactly this voluntary expectation of a certain direction consisted, or how we achieved it, we could not say; but the effect was often very definite.

This fifth series of observations, then, supports very strongly the indications afforded by the preceding series, to the effect that besides the two conditions of stimulation which certainly are of high importance for localisation (namely difference of intensity of stimulation of the two ears, and modification of the sound wave by the external ear in different ways according to the direction of its incidence) some other condition is also operative. This third condition must be monaural and therefore of the three other recognised possibilities, two only are in question, namely differential stimulation of the semicircular canals by sound waves coming from different directions and stimulation of the nerves of the skin of the meatus and tympanum.

There are many difficulties in the way of regarding either of these as cooperant conditions of localisation. One difficulty applies equally to both, the fact, namely, that this third condition (whatever it be) seems to be called into operation by complex sounds, but in much less degree or not at all by pure tones.

But the chief objection to assigning any rôle to the semicircular canals is the difficulty of conceiving how the vibration can be conducted to them through the bones of the head and the fluids of the internal ear in such a way as to stimulate the different canals differently according to the direction from which the sound approaches the head.

And the chief objection to attributing any importance to stimulation of the skin-nerves of the meatus and tympanum in monaural localisation is that it is difficult to conceive that such stimulation can vary with the direction of the source of sound in any other way than in intensity merely, and such variations could afford assistance only if the sound used for stimulation were of constant intensity. Nevertheless some very simple observations and considerations incline us to attribute importance to this last condition. First, that the skin of the inner part of the ear is extremely sensitive is proved by the well-known experiment of detecting the presence of a solid object held some few inches from one meatus. Secondly, we found that, when one ear was stopped and the other covered with wax forming an artificial meatus, although we could not distinguish pure tones on one side of the head from those sounded on the other side, we could always distinguish with the utmost confidence slight noises made on the right and left sides. This sure discrimination of right and left in the case of noises does not seem to depend upon difference of intensity of stimulation, for we found it impossible to deceive the subject by varying the intensity of the noise; and, when the noise was made on the side of the open ear, the subject was distinctly aware of some local sensation in the meatus[1] which was lacking when the noise was made on the other side of the head; and this local skin-sensation seemed to him to be the factor on which he based his judgment. It would seem then that this localised skin-sensation is excited in appreciable degree only by complex sounds made at some point on the side of the ear by which it is appreciated, and not in any appreciable degree by pure tones or by complex sounds on the side of the head remote from the ear affected. How these peculiarities of stimulation are to be accounted for, we cannot suggest; nor do we understand how such stimulation of the skin could render possible the judgments of direction that were given under the conditions of our fifth series of experiments. Therefore, although we are inclined to regard such stimulation of the skin as playing some part, we think there must be some further operative condition. Nor would such stimulation of the skin account for the results obtained in Series 3, for in the experiments of that series the meatus was closed on both sides.

We have one other observation to report bearing upon the problem of the conditions of stimulation operative for localisation. Lord Rayleigh has recently published experimental evidence which seems to prove that the localisation of a tone of deep pitch to the right or left side depends upon the difference of phase of the waves simultaneously reaching the two ears. He inclines to infer from this demonstration that the auditory nerves reproduce in some way the phase of the air-waves. This interpretation is so difficult to reconcile with our views of the nature of nervous process, that it seems right to look for some other explanation; and it seemed to us possible that the difference of phase might determine an intracranial interference of the waves propagated through the skull from the two middle ears and might thereby determine a difference of intensity of stimulation of the auditory nerves of the two sides, and that localisation of the deep-pitched tone might therefore be due to different intensities of stimulation of the two nerves produced in this way[2]. It seemed therefore of interest to determine the threshold of local discrimination for a pure tone of deep pitch. For if this threshold should prove to be of approximately the same magnitude as that for tones of higher pitch, that would support the view that localisation depends in both cases on the same condition of stimulation, namely difference of intensity of stimulation of the two nerves; whereas if the threshold for deep tones should prove to be of a very different order of magnitude from that of higher tones, that result would support Lord Rayleigh's interpretation. Owing to a variety of difficulties and the departure of one of us from England for an indefinite period, we were able to make only one satisfactory determination of a threshold for a pure tone of deep pitch. We used a fork of frequency 100, and a cylindrical brass resonator. By bowing the fork gently we produced a note which was quite inaudible to the subject at a distance of one metre, until the fork was held close to the mouth of the resonator, when the deep tone appeared to be free of overtones. Proceeding as in the first series, we determined the threshold at position *A* for one subject (W. McD.) and found that 25° is approximately the measure of it. The subject made his judgments with great uncertainty in all cases—owing perhaps to the very unfamiliar character of the tone; and this made it difficult to determine the threshold with accuracy; but it was quite clear that its value lies between 20° and 30°. The corresponding threshold of the same subject for the pure tone of higher pitch was 20°–. It would seem, then, that the thresholds for the pure tones of high and of deep pitch do not differ very greatly, but are of the same order of magnitude.

---

[1] Even when the noise was of minimal intensity.

[2] Profs. C. S. Myers and H. A. Wilson ('On the Perception of the Direction of Sound,' *Proc. Roy. Soc.* A. Vol. 80) have suggested this interpretation of the facts and have adduced experimental evidence in support of it. The neglect of the fact of intracranial conduction and interference led Prof. Sylvanus Thompson many years ago to infer from the fact that beats may be heard when two tones of appropriate vibration-rates are led separately to the two ears, that the auditory nerves reproduce in some way the phases of the air-waves. But it seems to have been fully established that these beats are due to intracranial interference (cf. especially K. L. Schaefer, in Nagel's *Handbuch der Physiologie*, Bd. III. S. 576, Braunschweig, 1905).

In conclusion we would venture some remarks on the bearing of our results upon the second of the two problems distinguished above, namely the problem of the way in which the operative conditions of stimulation are utilised in the formation of the judgment of direction.

We would insist upon a difficulty that has been, we think, too little regarded by those who have accepted difference of intensity of stimulation of the two ears as a principal condition of localisation. Lord Rayleigh has calculated the approximate intensity of the sound-shadow thrown by the head when a note of medium pitch is sounded at one metre from the head[1]. This shadow is the measure of the difference of intensity of stimulation of the two ears, when the source of sound lies at one side of the head in the line of the interaural axis. It would seem that the difference must, even in the case of tones of medium pitch, be very considerably less than 30 % of the total intensity, which is usually accepted as the approximate measure of the threshold of discrimination of intensity of sound. Our experiments (Series 1) show that in the case of pure tones (when apparently difference of intensity is the sole condition of discrimination) the position of a source of sound is perceived to be changed when it is moved some 20° only from the sagittal plane. The change in the intensity of the sound-shadow produced by such a change of position is, we imagine, very much less than the total value of the sound-shadow. It would seem then that the least difference of intensity of stimulation of the two ears that can determine judgment of direction is very much smaller than the least difference of intensity that can be appreciated as such. It is in accordance with this fact that the subject was not aware of hearing the pure tone with both ears, and of inferring its position from the greater intensity of the tone in one ear; but usually simply heard the one tone there or there. Occasionally it happened that the subject's answer was arrived at by way of an inference from an awareness that one ear was more strongly stimulated than the other, but in such cases this awareness seemed due to skin-sensation, or at least to sensation localised distinctly in the meatus of the more strongly stimulated ear. It seems necessary then to postulate some local signature of the tone-sensations determined by the unperceived difference of intensities of the stimulations of the two ears; possibly some reflexly evoked tendency for the face or the eyes to be turned to the source of sound, as suggested by Mr Matsumoto[1]. Such a theory of local signature seems strictly analogous to Lotze's doctrine of visual local signs, but escapes its greatest difficulties, because it has to deal with the localisation in one direction only at any one moment, and not with a sum of directions constituting an extended surface[2]. We venture to suggest also that such a theory might be rendered more acceptable, if we allow the local skin-sensations of the meatus to cooperate with movement-tendencies in the constitution of the local signs. Thus we should have a simple system of complex local signs comparable to the complex visual local signs of Prof. Wundt's theory.

---

[1] *Nature*, Vol. LVI.

---

[1] 'Researches on acoustic space,' *Studies from the Yale Psychological Laboratory*, Vol. v. 1897.

[2] Von Kries (*Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.*, Bd. I.) has argued that such a theory is unacceptable because he found it possible to localise correctly two simultaneously sounded whistles of different pitch on the left and right sides respectively. To us it seems that no difficulty is presented by this fact, unless the two sounds are not only simultaneous but also of minimal duration.

A difficulty for any theory is presented by the fact that the surprisingly accurate discrimination of the positions of noises and complex tones seems to be due to the cooperation of two or more conditions, each of which alone renders possible only an inferior degree of accuracy. This effective cooperation or summation of the two or more favouring conditions seems to us a very remarkable fact, and one very difficult to understand.

THE 'PERCEPTIVE PROBLEM' IN THE AESTHETIC APPRECIATION OF SINGLE COLOURS.

By EDWARD BULLOUGH.

[*From the Psychological Laboratory of Cambridge.*]

I. The Object: the 'Perceptive Problem'; the 'Aspects of Colour'; 'Perceptive Types.'
II. Experiments: Purpose, Material and Methods, Defects, Results.
III. Analysis of Records: A. the 'Aspects of Colour.'
B. the 'Perceptive Types.'
IV. Conclusion: the Aesthetic Values of the 'Perceptive Types.'

I. *The Object.*

THE object of this set of experiments is the investigation of a problem of colour-appreciation, more fundamental than the barren question whether certain colours are *per se* and universally pleasing, and more fundamental than the investigation into the part which tone, saturation and luminosity play in colour-preferences. For the problem is one which in the case of all aesthetic objects, not only of colours, lies at the bottom of, and conditions, our appreciation of them. It is what may be called the '*perceptive problem.*'

It appears that J. Segal[1] was one of the first definitely to call attention to the importance of this point, as a phase antecedent to the aesthetic contemplation. In his experiments with simple spatial forms he notices that the appreciation of an oblique straight line fluctuated according as the line was taken either as a badly drawn vertical or horizontal, or as representing the movement of the line to lift itself up to a vertical position or as flying along like an arrow. He observed these differences in the perception of the line both between different persons and in the same person at different times, and he lays special stress upon the obvious connexion existing between this initial perception and the ultimate appreciation of the object. This is a fact which obtains in our appreciation of other things as well, and the tendency to regard an object as 'meaning' one thing rather than another, cannot fail to produce divergencies in the attitude and appreciation of different subjects. It is true that in certain objects, e.g. those of Fine Art, the perceptive differences are far subtler and possibly much smaller than in the case of a straight line, since the very complexity of such objects stands in the way of considerable perceptive differences. Thus the mere addition of an arrowhead to the oblique line would at once have rendered Segal's figure quite unambiguous, and the human significance of statuary and painting still further minimises perceptive multiformity, though it opens the gate to other more complex factors of appreciation.

---

[1] J. Segal, 'Über die Wohlgefälligkeit einfacher räumlicher Formen,' *Arch. für d. ges. Psychol.* 1906, Bd. VII. S. 107.

In the case of colours this perceptive problem appears of paramount importance for two reasons. First, because the 'cognitive' value of colour is very low, and secondly, because it possesses a peculiarly high suggestive power which is only rivalled by odour in diversity and precision. Consequently a colour presents at once too few and too many indications to perception: any interpretation or meaning within the widest limits of fancy becomes possible. A colour may, therefore, 'mean' almost anything; experience seems to show that for no two persons does it carry exactly the same import; and it is consequently impossible to straightway coordinate the experiences of different subjects (though they are induced by one and the same object), simply because the colour already stands to the individuals for entirely different things, before any effects of like or dislike appear. Expressed in a general way, the perceptive problem in the aesthetic appreciation of single colours means *the problem of the differences existing in the perception of single colours, in so far as such perceptive differences condition differences of aesthetic effect.*

Translating this *subjective* formulation of the problem into the corresponding *objective* form, the question deals with what might be called the '*aspects of colour*.' What, for example, strikes one person, is the softness or brightness of a colour, or its 'objective' qualities; what appeals to another is its associative import. These are only two aspects; and the object of these experiments is in the first place to investigate what aspects actually occur in colour-appreciation, and to classify the evidence of the subjects with regard to this point, and secondly to endeavour to trace the origin of such aspects from the objective constituents of colour, tone, luminosity, and saturation.

It is conceivable that the same colour might impress the same person by several of its aspects. These aspects might reinforce or contradict each other in their feeling-tone, producing, in the latter case, indifference or fluctuations of appreciation. It will be seen, however, that owing to the genetic connexion of the main aspects with each other such complications rarely occur, as the several aspects, successively perceived, are compatible *inter se*, with the exception of the 'associative' aspect, which not infrequently exercises a disturbing influence upon the appreciation.

Such a competition between the various aspects appears besides to be *subjectively* excluded, by a more or less fixed habit of the subjects to favour one aspect rather than another. An attempt at classification in this respect revealed the existence of what I propose to call 'perceptive types' among the individuals. These differences of type would appear to be the fundamental reason for the much debated differences of appreciation of colour. They would represent in fact the ultimate point to which aesthetic analysis can penetrate in this direction. For the adherence to a special type seems to be a matter of general psychical constitution, the expression of the subject's whole mentality. Here lies the point of contact with the subject's personality, which not only makes itself felt in regard to colour-appreciation, but which determines his attitude to aesthetic objects as a whole, nay, which perhaps determines his attitude to things in general and his experience in the widest sense.

Incidentally it may be remarked that there was formerly, and in a sense there is still, some doubt about the aesthetic status of single colours. In the formalistic days, single colours were treated as merely agreeable, as a 'pleasing stimulation to the eye,' and were placed in the same category as pleasant scents or agreeable tactile sensations. This view, however strange it may appear, is partially supported by experience, for to certain persons single colours are never more than merely agreeable. On the other hand the large majority would not hesitate to call certain colours 'beautiful,' whatever Aesthetics might say to the contrary. It is only one of the many occasions on which this much debated problem of a distinction between the 'beautiful' and the 'agreeable' appears in speculative Aesthetics. Later, under the influence of hedonistic Aesthetics, the difference seems to have disappeared in theory, as both the 'beautiful' and the 'agreeable' were simply considered as different degrees of pleasantness without any qualitative difference between them. Still, an apparent qualitative difference continues to exist in practice, and most people susceptible to colour-effects are very decided in their distinction between the epithets 'agreeable' and 'beautiful' as applied to colour. Hedonistic Aesthetics with its 'pleasure-theories' failed very naturally to account for a difference which from the outset of its investigations had been obliterated; yet psychologically, the differentiation between the 'beautiful' and the 'agreeable' is perfectly feasible and appears closely related to the perceptive problem, as I hope to show in the succeeding pages.

II. *Experiments: Purpose, Material and Methods, Defects, Results.*

In 1904 I began and continued for some time a set of experiments which, however, entirely failed to accomplish the object which I had in view. Nevertheless they furnished certain data which served at the same time as a starting-point for a fresh set of experiments which were continued up to the end of last year. In view of the failure of the first set it would seem unnecessary to mention them, were it not that some of the results proved valuable from the point of view of the second series, and I shall therefore sketch with a few remarks the general drift of the earlier experiments. They were undertaken with a rough notion of Fechner's 'direct' and 'associative' factors of colour-effects, and I had hoped by a comparison of the effects of single colours under prolonged and instantaneous exposure to separate these two factors, on the assumption that the instantaneous exposure of the colour would not give the subject time to develop the 'associative' effect. This expectation, however, proved illusory, and the experiments failed completely in this respect. I had used in these experiments two series of eleven colours in each, produced by gelatine plates, two or more superimposed to obtain sufficient variety of tones. The experiments took place in a dark room with the help of the apparatus illustrated in the previous number of this Journal[1]. The light from two incandescent gasburners was transmitted through the gelatine plates and through a large lens, so that the desired colour appeared as a circular patch on an opal glass placed before the subject. Instantaneous exposure was produced by a large pendulum bearing a wooden screen, which swung behind the opal glass across the line of the light-projection, and allowed the colour to pass through an aperture in the screen, thus letting it appear for a fraction of a second on the opal glass. Each series of colours was exposed twice, once as long as the subject wished, and once instantaneously, so that each subject was tested with 44 colours in all. There were eight subjects, some of whom figure also in the second set of experiments. In order to distinguish the two sets, the subjects of the first (given in abridged form) will receive the letters A—H, the subjects of the second set being represented by numbers, 1, 2, 3, etc. Eventually I abandoned this proceeding as it did not seem to lead to any results.

---

[1] *Brit. Journ. of Psychol.* 1907, vol. II. p. 245.

In 1905 I started on the second set of experiments with entirely different material. Instead of gelatine plates, coloured paper was used; instead of transmitted light, reflected light from two incandescent burners with strong reflectors. For the colour-material I employed the fully saturated colours made for optical experiments, and (for broken and mixed shades) wall-papers and various papers used for book-binding purposes. From about 100 different colours and shades I selected 70 for the experiments, but in course of time I discarded half of these, as the mass of registered data became unnecessarily unwieldy and the actual colour-material turned out to be really the least important part of the investigation. Specimens of the 35 coloured papers used are given in the accompanying plate.

The experiments took place in a dark room in order to avoid the variations of daylight. The apparatus consisted simply of two screens placed parallel to each other at the short ends of an oblong table. In the first screen was a circular hole of 5 cm. diameter, which could be closed by a shutter. To this hole a narrowing funnel was attached, extending nearly up to the eye of the subject sitting in front of this screen, and through this the colour could be seen upon the second screen opposite. The coloured papers were pasted in big square strips upon pieces of cardboard, which were placed upon the second screen, so that the subject saw only a disc of colour through the funnel and the circular hole in the first screen. In order to fix the position of the subject's eye a chin-rest was screwed to the table a little to the left of the eye-hole so that the subject's right eye was opposite the funnel. The left eye was covered by a small black screen to prevent squinting, and the fatigue consequent on trying to look with both eyes through the hole. Unless attention was specially directed to it, the subject often remained quite unconscious of the fact that he was using only his right eye, and though the use of monocular vision might appear at first sight as a serious defect, it proved in reality a considerable advantage. For the use of only one eye helped to a large extent to hide minor irregularities in the surfaces of the coloured papers such as wrinkles, small differences of texture, &c., and left also the actual distance (84 cm.) between the two screens more or less uncertain. It did not entirely do away with it, which proved to be another advantage, for, although the colour was seen actually on a black background, namely the black inside rim of the circular hole, the obviously intervening distance tended to prevent the subject from regarding the colour and the black setting in the form of a colour-combination. This seems to have been a complication in most experiments, in which the colour and the setting are shown on one and the same plane. The colours were not shown in any fixed series relatively to their values or tones. The arrangement was, on the contrary, quite arbitrary, except that all the subjects were tested with the same colours in the same sequence. In all, thirty-five persons were subjected to the experiments—three of them were women, the rest, with two exceptions, were graduates and undergraduates of this University. Certain differences in colour-appreciation, quite obviously due to the mental training and to the influence of the particular studies and preoccupations of the subjects, I shall have an opportunity of pointing out in the analysis of the results. As regards the method of procedure, two points require special comment, in so far as they form a special feature of these experiments.

The subjects were instructed first to give *single judgments*, not *preference-judgments*; and secondly, as far as possible, *introspective* evidence, especially in regard to the subjects' reasons for their positive or negative judgments. While attaching very little weight to the judgments given on each individual colour, I consider it as of the utmost importance *not to demand preference-judgments* from the subjects. I am inclined to attribute to this latter proceeding the generally very poor results which have so far been obtained from aesthetic experiments with colours or, for that matter, with other simple objects. This is the case, for instance, in Cohn's experiments[1], and, for a different reason, in Major's[2]. The result of Cohn's experiments, that saturated colours are generally preferred, and that, where this is not the case, the reason is probably to be assigned to association, can hardly be said to do justice to the extraordinary subtle and many-sided effects of colours. It appears to me, on the contrary, quite obvious that the subject's attitude and his appreciation were forced in the direction of one particular aspect of colour, namely saturation, as the demand of a comparative judgment would naturally call his attention to the most evident and most easily observed objective feature of the two colours under consideration. It is true that Major asked for *single* judgments, but these single judgments are really only apparently single, for with his peculiar method of presenting his colour-series in different stages of saturation he must have forced the attention of his subjects into exactly the same line of observation as Cohn did with his method of comparative judgments, although he disagrees with Cohn as to the general preference of saturated colours[1]. Quite apart from these considerations, to demand preference-judgments seems to me to be fundamentally and radically opposed to the aesthetic attitude in general. The method of comparison has been taken over wholesale from purely psychological experiments, where it served special purposes, into aesthetic experiments, where it destroys the pre-adaptation of the subject to aesthetic experiences, and thereby vitiates his whole mental attitude towards the objects to be offered to his appreciation. It is precisely characteristic of the aesthetic appreciation to be non-comparative, individualising, isolating and, in a sense, absolute. Whenever therefore I noticed a tendency of certain subjects to preference-judgments, I discouraged it as much as possible, though they occasionally appear in almost all the records in the shape, for instance, that a subject likes one colour better than some previous one.

---

[1] J. Cohn, 'Experimentelle Untersuchungen über die Gefühlsbetonung der Farben,' *Phil. Stud.*, Bd. x. 'Gefühlston und Sättigung der Farben,' *Phil. Stud.*, Bd. xv.

[2] Major, 'On the affective tone of single sense-impressions,' *Amer. Journ. of Psychol.*, vol. VII.

In fact, from the very beginning I endeavoured to impress upon the subjects that it was less their *opinion* of the colours that I wished to obtain than the *reasons* which prompted their judgment. I wanted to know what impressed them in particular in each colour, how each one appeared to them, by reason of what special features they liked or disliked it; in short, what was happening in their minds in reference to each particular colour. The simplest way of getting this information was to ask whether they liked the colour, and, if so, why they liked it, or if they disliked it, to ask for the reason for their dislike. The main object of the entire investigation was to get their introspective evidence with regard to the colour; the judgment being merely a means, and the easiest way, of entering into their mentality.

---

[1] *Loc. cit.* S. 67–77. Possibly with a larger number of subjects the results of this forcing would have been more apparent.

This introspective evidence has been much discredited. Thus Cohn says[1] in reference to some of his experiments: 'The subject is supposed to let the sensory impressions act upon himself, as far as possible, without reflecting on them, and then to formulate this purely affective process in the shape of an intellectual judgment. There is, therefore, a danger that this intellectual judgment is in a sense given precedence, that the subject begins to theorise and unconsciously forms his judgment according to these theories.... [To obviate this tendency] one must...not encourage the subject to introspection. The whole process of judging must become to some extent mechanical, so that finally the affective impression is that part of the experience which reaches the consciousness of the subject in the most definite form.' Apart from the questionable theory implied in Cohn's conception of the appreciative process[2] (for to render this process mechanical robs it of all its evidential value), the purpose of my experiments was quite different from that of Cohn. To get at the aspects which colour may present to different individuals, their introspective evidence was inevitable. Nor have the results proved its untrustworthiness. Even granted that there occurred a certain amount of self-suggestion, there is a large residue of very circumstantial evidence which appears perfectly spontaneous and was obviously the direct statement of facts, not the result of self-made theories or carefully considered conceptions. It is true that in some cases the subjects spoke with a kind of dogmatic assurance which pointed to very firmly established notions on their part as to what colours *ought* to be like. They evidently had a theory and a scale of values which they unhesitatingly applied to all colours, but this does not seem to me to vitiate the evidential value of their introspection. It merely proved in itself an interesting point, viz. that there *are* people who actually and habitually approach a colour in this manner. It seems to be a part, an acquired part no doubt, of their psychical constitution, and throws an interesting light not only on the relation in which they appeared to stand to an aesthetic object, but also on a large class of aesthetic appreciations, those, for example, which are mainly based on what might be called 'conformity to type.' I am convinced that Cohn's method has very seriously aggravated the artificiality of the already very artificial conditions of such experiments.

---

[1] *Loc. cit.* x. S. 596–7.

[2] Compare also the criticisms directed against Cohn's experiments on the same grounds, by J. Larguier des Bancels, *Année Psychologique*, 6^{me} Année, 1900, pp. 176, 177.

While this paper was in the press, a well-formulated and incisive criticism of hedonistic aesthetics and its experimental methods appeared in *L'Esthétique Expérimentale Contemporaine* by C. Lalo, Paris, 1908 (pp. 88, 116, 152–156, 172–182).

The subjects varied very much in the readiness with which they gave their introspective evidence. In some cases it required a little time and practice to make them realise what was really wanted of them, but none can be said to have altogether failed. They usually understood very quickly the general drift of the tests, and soon volunteered their statements without any questions being put to them. I refrained, of course, most carefully from putting any pressure whatever upon the subjects. Rather than force an answer, I contented myself with leaving blanks in the records. Especially in view of the sometimes rather tedious procedure and the strain it must have been for the subjects, I should like to take this opportunity of thanking them again for their kindness in submitting so readily to this, sometimes not easy, task and for the valuable help they thereby rendered me during these tests. My special thanks are due to Dr Rivers for the kindly encouragement, advice and assistance he has given me throughout the experiments.

In order to meet at the outset certain objections, I should like to point out several *defects* which became apparent in the course of working. Some might have been avoided, at least with partial success; others, on the other hand, were practically inevitable under the conditions under which the experiments were made. Among the first must be reckoned the disadvantage which arose from the smallness of the area of the exposed colour (a disc of 14 cm. diameter). I believe that in all cases considerably larger surfaces would have produced more decisive results than were obtained from a circular patch of only 14 cm. diameter. Possibly many rather indefinite effects would with an area of, say, 1 m. diameter, have become so marked and emphatic as to leave no doubt in respect to their character in the mind of the observer. Unfortunately the space at our disposal prevented the use of colour-surfaces of such a size, even if there had been the means for procuring adequate illumination. Even with the small disc employed, the illumination gave much trouble and can be made directly responsible for another serious defect. From the list of the colours (Table II) it will be seen that there is a very strong predominance of yellows over the other tone series: there are 11 yellows, as against 7 reds, 7 blues, 4 oranges, 3 greens, and 3 purples; the complete absence of browns is also to be noticed. Both these points are traceable to the insufficient illumination. In the absence of electric light and after some unsuccessful trials with acetylene gas, I was obliged to use incandescent gas; but in spite of its apparent whiteness it contains still far too much yellow to allow of many shades of good reds, greens, blues or purples. The tone of all these series is badly spoilt by the admixture of yellow light; the reds, unless markedly bluish, all tending to orange, the greens to a sickly yellow-green, the blues to green-blue, and the purples to pink or dull brownish shades. Only genuine yellows and oranges and a few reds and blues retained some of their tone value and brilliancy, but even these, and still more the other colours, showed an extraordinary decrease of tone value. Hence the complaints about the unpleasantness of so many colours, and the frequent cases of hesitation in regard to the tones, as the colours appeared so often mixed and indefinite. I do not believe that these defects of exposed area and of tonality actually invalidated the results obtained; they only weakened the effects, but do not seem to have materially altered the perceptive processes involved in the appreciations, positive or negative, of the colours. I shall call attention later to peculiarities in the records which I think are traceable to these defects. Their discounting will only tend to confirm the general conclusions which the analysis of the records renders probable.

A practically inevitable defect lay in the general experimental conditions imposed upon the subjects merely by their surroundings, by the procedure and by the ambient atmosphere of scientific investigation. It is a drawback under which all aesthetic experiments are bound to suffer and which can only in a slight measure be relieved by the combined efforts of the observers and the experimenter. It was, of course, impossible to hide from the former the fact that they were making observations; and this together with other factors tended to produce a defective or wrong initial adaptation in some of the subjects, inasmuch as they felt called upon to state rather what they *thought* about the colours than what they *experienced* in reference to them. This tendency was no doubt to some extent aggravated in the first place by the defects of the material as stated above, and secondly by the subjective attitude of several observers which appears to have been the result of their special training or study. For many of them were students of natural science, trained and accustomed to objective more than to introspective observation; sometimes rather proud of their accomplishments in this direction and at first unwilling to pay attention to their own impressions rather than to the objective features of the colours. Explanations as to the purpose of the tests and questions about their personal experiences helped to some extent to bring them to the required point of view; still the difficulty

TABLE I. *Abridged Records of older set of Experiments.*

	COLOURS	A.	B.	C.	D.	E.	F.	G.	H.
1	red, 1	*pl.* 'fine colour'	*pl.* plenty of colour in it	*pl.* sun in a fog	*dist. pl.* full	*dist. pl.*	*ind. unpl.* sickly	*pl.*	*pl.* deep nature, majestic, severe
2	purple, 2	*pl.*	*ind. unpl.*	*ind. unpl.* might have been nice, spoilt by blue	*pl.*	*ind.*	*pl.* pontifical, soft	*ind. unpl.* difficult to grasp	*pl.* delicate, light, airy
3	pale yellow	*unpl.* 'poor col.'	*dist. unpl.* indefinite	*unpl.*	*dist. unpl.* washed-out	*unpl.* tarnished gold	*dist. unpl.* 'mud'	*dist. unpl.* very dirty	*sl. pl.* weak, delicate, interesting
4	violet, 2	*ind.*	*ind. pl.*	*ind. unpl.*	*ind.* purple and lilac, muddy	*ind. pl.*	*ind. pl.* suggests purple	*sl. unpl.* colours not quite fused	*dist. pl.* noble, high, majestic, serene
5	bluish red	*pl.* well saturated	*pl.* decided	*sl. pl.* cf. 1	*ind. pl.* ,,	*pl.*	*dist. unpl.* irritating, glaring	*ind.*	*ind. pl.* no character, not deep enough
6	orange, 1	*ind. pl.* well sat.	*pl.*	*sl. pl.* not pure enough	*ind. pl.*	*unpl.* too light	*unpl.* 'objectionable sweetmeats'	*unpl.* orange series	*dist. pl.* luscious, cheerful, not profound
7	salmon-pink	*ind. unpl.* unsat.	*unpl.* washy	*sl. unpl.*	*unpl.* washed-out	*unpl.*	*unpl.* suggests brown	*unpl.* physical illness	*pl.* delicate
8	yellow-red	*pl.*	*pl.* decided	*sl. pl.* but some blue in it	*dist. pl.*	*unpl.* 'gone past red'	*sl. pl.* rich, 'Pears' soap advertisement'	*pl.* warm, rich	*pl.* cf. 1, self-centred
9	red-purple, 2	*dist. pl.* well sat. 'pretty col.'	*pl.*	*sl. pl.* spoilt by blue	*pl.* 'purple-shading'	*ind. unpl.* ought to be darker	*dist. pl.* assoc. ecclesiastical vestments	*pl.* ,, ,, more than 8	*pl.* velvety softness
10	bluish green, 2	*unpl.* very poor	*unpl.* bad light	*unpl.*	*unpl.* muddy	*unpl.* too dark	*dist. unpl.* 'railway-signal in fog'	*unpl.* bad illumination	*ind. pl.* mysterious, incense smoke, sl. sickly
11	red-purple, 3	*pl.*	*ind. pl.* too much blue in it	*sl. pl.*	*pl.*	*unpl.* 'Condy's Fluid'	*dist. pl.*	*pl.* warm, rich	*pl.* too dark, blind
12	orange, 2	*pl.* sat. colour	*ind.*	*dist. pl.* sun in fog	*unpl.* dull, heavy	*ind. unpl.*	*unpl.* 'Trying to be what it isn't'	*ind.*	*ind. unpl.* irritating
13	blue-purple, 1	*ind.* weak, unsat.	*unpl.* watery	*ind. unpl.*	*pl.* approach to favorite col., lilac	*dist. unpl.* 'sky in bad weather'	*pl.* 'pontifical vestment,' 'amethyst'	*pl.* rich	*ind.* not as rich as 12
14	yellow	*pl.* 'good yellow'	*unpl.* green in it	*ind. unpl.* too green; pers. assoc.	*ind. unpl.* insipid	*unpl.* pers. memory	*ind. pl.* 'garden, wild flowers'	*sl. pl.* brightness	*dist. pl.* exhilarating, lightheartedness, sunshine
15	orange, 3	*dist. pl.* 'beautiful,' 'nice col.'	*ind. pl.*	*pl.*	*ind.* melancholic, heavy	*pl.* 'sun-set'	*unpl.* 'blood'	*ind. pl.* exciting	*ind. pl.* severe admiration, unapproachable
16	olive-green	*pl.* 'good col.'	*ind. pl.*	*ind.*	*pl.* soothing	*ind. unpl.* unhealthy green in nature	*sl. pl.*	*dist. pl.* soothing, rich	*ind. pl.* not bright enough
17	red-purple, 1	*pl.*	*pl.* richer than 16	*pl.* cf. 12	*dist. pl.*	*dist. pl.* 'red flax'	*sl. pl.* faint ecclesiastical suggestion	*sl. pl.* pl. richness, unpl. brilliancy	*dist. pl.* velvety softness, not severe
18	bluish red	*pl.*	*ind.*	*ind. pl.* a little sulky	*pl.* tends to lilac	*unpl.* assoc. with war, unrest	*ind. pl.* sickly, sentimental	*pl.* red, less bright	*ind. pl.*
19	green, 1	*pl.*	*pl.* likes greens	*ind. pl.*	*unpl.* too bright	*unpl.* badly nourished vegetation	*sl. pl.* calming	*dist. pl.* rich, pure colour	*dist. pl.* brightness, sunshine, softness
20	violet, 1	*pl.* 'nice colour in its way'	*dist. unpl.* no body in it	*unpl.*	*pl.*	*unpl.* insipid	*pl.* transcendental suggestion, rest, fulness	*dist. unpl.* thin, undecided, characterless	*pl.* very soft
21	purple-orange	*unpl.* not so bad after consideration	*ind. unpl.* indefinite	*ind.* mysterious fluctuation of judgment	*ind. unpl.* dull, heavy	*ind. pl.*	*unpl.* very sickly	*ind. pl.*	*ind. pl.* frowning
22	green, 2	*pl.* as sat. colour	*pl.*	*ind.*	*pl.* not depressing	*pl.*	*pl.* soothing, restful, 'sun on the grass'	*dist. pl.* cf. 19	*pl.*

A. No preferences. Saturation = index to pleasingness.
C. *Pref.*: reds, yellows, oranges for their warmth. *Dislikes*: Blues for their cold.
E. Says the associations are extremely precise, often identifies name of persons with definite colours. Conscious of forming assoc. with objects actually perceived. *Pref.*: green: botanist.

persisted in the case of some subjects and may even to some extent have been natural or at least so deeply ingrained as to represent their normal attitude; especially in the case of those who acknowledged themselves to be more or less insusceptible to colour-effects. Personal acquaintance with some of the persons of this type led me, however, to be cautious in drawing inferences from such results, as I knew them in ordinary life to be highly appreciative of colour. Under these circumstances there can be little doubt that one or other or all the disturbing factors, objective and subjective, had interfered with the proper development of their appreciation in the tests. This is a point which will be discussed later with illustrations from the records.

After these preliminary remarks I give the records in tabular form and proceed to their analysis.

III. ANALYSIS OF RECORDS.

A. *The Aspects of Colour.*

Even a quite superficial glance at the records obtained from the tests at once reveals certain differences in the attitude of different subjects taken up towards the colours. These divergencies betray, on the one hand, subjective differences in the perceptive processes leading to the appreciation[1]; on the other hand, they represent what I have called (page 407) 'aspects of colour.'

Thus No. 1, No. 5, No. 22, and No. 35 give us four instances of such differences in a state of relatively high purity. No. 1's remarks refer almost exclusively to such peculiarities of colour as its saturation, its brightness, its delicacy, its poorness. No. 5 comments predominantly on the associative import of the colours: they remind her of sunsets, of flowers, of trees, of the sea; while No. 22 frequently complains of the coldness of the colours, or rejoices in their warmth, their restfulness, pleasing coolness or softness. Lastly, No. 35 seems to be impressed with certain characteristics of colour, which, in default of some better expression, might be termed their 'Character' or 'Temperament'; he describes colours as being insipid, stubborn, treacherous, energetic, jovial, self-confident, &c., all attributes which he applies to them in much the same way as if he were speaking of actual persons of his acquaintance. In short, they illustrate by the four main aspects of colour:

---

[1] The term 'appreciation' is here and in the sequel used in a specially restricted sense; it means the mere 'recognition of a value,' without implying the acceptance or rejection of this value ('positive' or 'negative' appreciation). To 'appreciate' a colour is therefore equivalent to understanding or grasping its 'meaning' or significance or value, quite apart from the question of whether this meaning is liked or disliked. In so far 'appreciation' represents a process antecedent to approval or disapproval, but naturally conditioning the ultimate attitude towards the colour.

(I) A group, which represents the *Objective Aspect*, such as those mentioned by No. 1.

(II) The 2nd group, which represents features producing certain effects on the subject, such as stimulating, soothing, or temperature effects, an aspect which for want of a better term might be called the '*Physiological*' *Aspect*[1].

(III) The 3rd group represents the suggestive power of colour, or its *Associative Aspect.*

(IV) And the 4th group the very subtle and divergent features of 'temperament' or 'character': the *Character-Aspect.*

I hope to show that all the various forms of aesthetic colour-impressions can be brought under these four divisions and that these four groups of colour aspects correspond to different perceptive processes, which in their turn condition the divergencies of aesthetic colour-appreciation.

Groups I and II: The 'Objective' and the 'Physiological' Aspects.

At the present moment it will be more convenient to treat these two groups together under one heading, although in the sequel the reason for the distinction between them will become evident. Let me first classify the various qualities ascribed to colour under these headings, before discussing their origin.

1. One of the oldest criteria for colour-appreciation is represented by the antithetical pair, *pure* and *impure.* Besides these there are the following epithets paraphrasing them more or less: crude, mixed, well or badly fused, dirty, mirky, foggy, rich, &c., or equivalents of these.

2. Expressions referring to certain effects on the subject: *soothing,* restful, calming, soft, depressing, mournful, flat, melancholic: or, *exciting,* exhilarating, stimulating, cheering, cheerful.

3. Terms like *warm,* hot, glowing; or, *cold,* cool, frigid, &c., and such like.

---

[1] For the reasons which led me to adopt this term see pages 422, 423.

4. All attributes derived from the impression of the *strength* of colour, such as strong, intense, violent, aggressive, offensive, irritating, sometimes also rich.

5. Expressions such as a '*good colour*,' a 'fine colour'; colours described as poor, thin, washy, dingy, watery, empty, indefinite, undecided, insipid, bilious, sickly; or as pronounced, decided, definite, full, having body, &c.

6. Closely allied to these are the epithets, *heavy*, solid, depressing, gloomy, melancholic; or, *light*, cheerful, delicate, transparent, translucent, diaphanous, ethereal.

7. Lastly, colour may be described as *bright*, glaring, brilliant, dazzling, cheering; or, as *dark*, sombre, gloomy, melancholic, opaque, dead, &c.

It will be noticed that occasionally these seven subdivisions seem to overlap each other. Thus the feature of cheerfulness is found in subdivisions 2, 6 and 7. The reason, as will be seen, is merely the verbal inadequacy of our language which by a natural extension of meaning describes in identical terms aspects, which, though of different origin, are similar in their effects. Genetic considerations oblige us, however, to ascribe occasionally seemingly identical features to different subdivisions. These seven subdivisions are, I submit, referable to the three elements of every colour, namely, its tone, saturation and luminosity, in such a way that one or other of these elements, either singly or in combination with one or both the others, is responsible for each individual aspect. The distribution would seem to be as is shown in the subjoined table:

TABLE III.

I. *Tone:* (*a*) Purity 1 (subd. 1).
(*b*) Stimulation (subd. 2).
(*c*) Temperature (subd. 3).
II. *Saturation.* Purity 2 (subd. 5).
III. *Luminosity:* (*a*) Heaviness (subd. 6).
(*b*) Brightness (subd. 7).
IV. *Saturation + Luminosity* } Strength.
V. *Tone + Saturation + Luminosity* } (subd. 4).

I offer a few remarks on each of the seven subdivisions in succession.

1. *Purity* 1. In spite of the antiquity of this aspect, there is peculiarly little unanimity as to its nature or its meaning, whether it

be regarded from a theoretical or practical standpoint. This uncertainty is largely due, it seems to me, to the fact that purity is referable to two different elements of colour, namely (as I tried to express it in the table), on the one hand, to the *tone-element*, and, on the other, to *saturation*; in other words purity may mean two different peculiarities of colour, either *purity of tone* or *purity of saturation.* This latter point will find its place under subdivision 5.

Purity of tone is quite obviously what is meant by Ruskin, when he remarks[1] that 'the original notion of this quality is altogether material, and has only been attributed to colour, when such a colour is suggestive of the condition of matter from which we originally received the idea. For I see not in the abstract, how one colour could be considered "purer" than another, except as more or less compounded, whereas there is certainly a sense of purity or impurity in the most compound and neutral colours, as well as in the simplest, a quality difficult to define.' After some discussion he comes to the conclusion[2] 'that the quality which we term purity is dependent on the full energising of the rays that compose it; of which if in compound hues any are overpowered and killed by the rest, so as to be of no value or operation, foulness is the consequence; while so long as all act together, whether side by side, or from pigments seen one through the other, so that all the colouring matter employed may come into play in the harmony desired, and none to be quenched or killed, purity results.'

However obscure Ruskin's explanation of the impression may be, this account would seem to correspond, on the whole, to the impression which a 'pure' colour produces. The ordinary individual, when asked what a colour must be to be called pure, would certainly answer that the colour should be '*colour*' and nothing else, that is, it should contain no admixture of any kind. But admixture of what? There lies the difficulty. On the one hand, the colour should be free from an admixture of grey or black; in so far as it is a question of saturation pure and simple. On the other hand, it should be free from admixture of any other *tone*; and there arises one complication which tends to render the whole point of tone-purity so vague and uncertain, namely, the inevitable interdependence of the three elements of colour. It seems that a colour is frequently called 'impure' when mixed with another tone, *if the second tone tends to reduce the luminosity or saturation of the original tone.* Thus though a pure crimson does contain a tinge of blue, it may still be considered pure, but a larger addition of blue, producing a tendency to purple, may be objected to as an 'impurity,' if it is regarded as unduly reducing the saturation of the red. Similarly a pronounced trace of green in yellow is frequently considered as a loss of purity, and the same is the case with a yellowish green, where the green seems to be watered down by the yellow, or with some blue-greens in which the decrease of luminosity from a pure green is very marked. Especially in naturally dark colours, such as brown, or in a bad illumination, the result of such mixtures is the production of colours, which are described as 'badly fused,' 'foggy,' 'mirky,' 'dirty,' 'muddy'—all expressions referring to the loss of saturation or luminosity. There are, on the other hand, strong, obviously compounded, colours such as a well saturated orange, or a brilliant intense purple which may perfectly well be called pure, because the combination of tones may not be felt as 'spoiling the colour,' provided the luminosity is good, and the saturation keeps to a relatively high level.

---

[1] J. Ruskin, *Modern Painters*, vol. II. pp. 81, 82, fourth, small edition (Allen, London).

[2] *Loc. cit.* p. 85.

The second complication is of a subjective nature. It is the well-known problem as to our sensations of single and compounded colours. Helmholtz[1] insisted emphatically on the singleness of the sensory aspect of such compounded colours; Professor Lloyd-Morgan[2] asserts his own inability to analyse the sensation orange into red plus yellow, while admitting that there are differences of opinion; Sir D. Brewster and Goethe maintained that they could see both blue and yellow even in green, which to most people is decidedly a single colour. It is, of course, unquestionably possible to separate *intellectually* the blue and the yellow in green, *a fortiori* the red and yellow in orange and the blue and red in purple, and this is where the *subjective* complication about purity lies. For the appreciation of colour with regard to this aspect is apparently based upon the *intellectual discrimination* of the compounded tones and upon some kind of *mental comparison* between a given colour and some standard of purity which the subject has formed for himself. Both these proceedings are noteworthy, and characteristic of the general attitude taken up by individuals of the objective type in respect to colour generally, as will be shown later. It may be remarked, by the way, that the judgments of different persons, as to purity, very naturally fluctuate a good deal, both on account of differences in the standards implied in the mental comparison, and because different persons are not *naturally* equally inclined to discriminate the compounded tones. This would also explain the apparent contradiction of calling a bluish red 'impure,' while reddish purple might be considered 'pure'; for purity is essentially a relative term which depends very largely both on the notion of purity which the individual employs as a standard, and on the natural standard of purity of each colour-series, *i.e.*, the highest possible degree of pure tone-value of which the particular colour-series is susceptible. A bluish red may be impure for a red, whereas a red-purple may be relatively pure for a purple. As a rule it may be said that purity is hardly ever a *positive aspect* of colour, in spite of the positive appearance of the term; generally it means simply the relative absence of impurity.

---

[1] Helmholtz, *Physiologische Optik*, 2te Aufl. S. 311, 312.

[2] C. Lloyd Morgan, *Introduction to Comparative Psychology*, pp. 132, 133.

2. *Stimulation.* The effects described as 'stimulating' and 'soothing' are evidently traceable to the *tone*-element of colour. That red is an exciting colour is well known, not only in the case of human beings, but also in the animal world; yellow, with the intermediate orange, shares, though in a lesser degree, this stimulating aspect with red. The blues, on the other hand, and to some extent the greens, are frequently felt as restful, soothing, inhibitory, even as depressing. With subjects, especially sensitive to this aspect, the impressions often reach an astonishing degree of subtlety, and the faintest trace of blue in a brilliant red, for instance, affects at once the exciting character of the colour. While in respect to blue the general opinion agrees as to its soothing effects, it fluctuates a good deal respecting green, which is perhaps best described as 'gently stimulating[1],' though it may tend more to stimulation or to restfulness according as the subjects detect tinges of yellow or blue in it. Still greater uncertainty can be observed in the case of purples. As a rule, purples are not felt to be either stimulating or soothing; this is an attribute hardly ever applied to them, except occasionally to blue-purples which are then described as depressing or melancholic, an effect undoubtedly due to the predominance of the blue. Red-purples, on the other hand, present the curious contrast of the combination of an obviously stimulating (red) with an undeniably depressing (blue) colour, and thereby produce an effect which places them generally outside this aspect altogether.

Although the facts of this 'stimulating' aspect are so well known, we must frankly confess our ignorance as to the nature or reason of these effects. In all probability they are of a physiological (perhaps chemical?) character, and this it is that has led me to treat 'stimulation' as a 'physiological' aspect of colour. I do not, however, wish to have thereby prejudged the problem as to the nature of its action in any way. It is interesting to remember in this connexion the experiments of Féré[1] on the 'dynamogenic' value of coloured lights, which varied with the tone of the colour shown: a hysterical subject, the normal strength of contraction of his hand being represented by 23, exhibited degrees of strength expressed by 24, 28, 30, 35 and 42 under a blue, green, yellow, orange and red light respectively. James[2] mentions this observation as an illustration of the usual reinforcing of muscular action by a simultaneous sensation; the differences in the dynamogenic value of the tones nevertheless require explanation.

---

[1] Ch. Féré, *Travail et Plaisir*, Paris, 1904, p. 105, 'Le vert...donne dans nos expériences le maximum de stimulation avec le minimum de fatigue dans un temps limité.'

3. *Temperature.* The distinction between *warm* and *cold* colours is as generally admitted as the stimulating effects of certain colours. The red-yellow series is universally recognised (though not universally *felt*) as warm; blue, especially in an unsaturated state, as cold. Green varies in its temperature aspect in the same way as with respect to its stimulating effect; *i.e.* it is doubtful whether it is more frequently felt as cool or as warm; in any case it is neither hot as red, nor cold as blue. Again, as with stimulation, the addition of a warm or cold tint to another colour results in the alteration of the temperature, inasmuch as any admixture of red or yellow has the effect of raising it, while the addition of blue lowers it very sensibly. Thus white may become warm, if slightly tinged with yellow, but exceedingly cold, if in the least degree bluish. Even essentially warm colours, as red, are markedly cooled down by the presence of a little blue, as for instance, crimson appears much cooler than scarlet.

Concerning the explanation of this temperature aspect, there has been a tradition of long standing which accounted for it, together with all other colour-effects, by 'association'; the colours round about orange were supposed to be warm by suggestion of the sun or fire, while blue was assumed to be associated with the appearance of ice, water, steel, &c. Even an 'ancestral memory,' an atavistic throwing-back to the times of our ancestral forefathers, to whom sun, fire and ice, warmth and cold were of much more vital importance than to us, has occasionally been appealed to for an explanation. Against such a theory, it is to be urged first, that the maximum of warmth is represented by red, and not by orange or yellow which in appearance would much more closely correspond than red to the fire or the sun; that secondly the striking similarity of the stimulation to the temperature aspect offers an assumption that both belong fundamentally together, and that both are of physiological origin; and lastly,—the most cogent disproof of the associative origin of the temperature-effects—that persons belonging to the 'associative type' (see below) are quite insensible to the temperature aspect, unless such temperature-effects happen to be suggested by the associative contents connected with the colours[1].

---

[1] Ch. Féré, *Sensation et Mouvement*, Paris, 1887, p. 42. A more detailed investigation of the influence of colours on work (with ergograph) is given in the same author's work, *Travail et Plaisir*, Paris, 1904, pp. 88–99. In this volume a collection of observations by other workers will be found on pp. 99, 100.

[2] W. James, *Principles of Psychology*, 1901, vol. II. pp. 379, 380.

I would therefore submit that the true origin of this aspect, as of the aspect of stimulation, is to be sought in the physiological action of the colours; and that both are fundamentally identical, the temperature-effects being really only a particular form of the general effects of organic excitation or inhibition.

4. *Strength.* The same difficulty attaching to purity, attaches also to strength, inasmuch as the strength of a colour is referable to two different combinations of objective qualities, and really represents two slightly different effects.

On the one hand, we call strong, intense, vivid, even aggressive, certain brilliant shades of red, scarlet, crimson, especially magenta; also of orange and even yellow. On the other hand, colours like a dazzlingly intense blue, a virulent green, a very vivid blue-purple, and even certain shades of brown, may be felt as strong, but apparently for a different reason.

It would seem, in fact, that strength in the *first* meaning is, so to speak, the *objective* interpretation of what, *subjectively*, is felt as a stimulating effect of high intensity. It is the red-yellow series which exhibits this strength, if combined with a high degree of brightness and the maximum of saturation compatible with this luminosity. It would, consequently, correspond to *tone* + saturation + luminosity.

The *second* kind of strength, on the contrary, is independent of the tone-element, as it can be attributed to practically any colour, even to such inhibitory shades as blue, provided the colour possesses the maximum of saturation plus the highest possible degree of luminosity.

It may be asked: why make a special point of this aspect of strength, since it might, in its former character, be brought under the heading of stimulation, and in its latter meaning, possibly under that of purity? This would, however, seem to me to be theorising without due regard to the *subjective* conditions, under which these epithets are applied to colour.

---

[1] That the temperature-effects of colours have nothing to do with actual temperature-differences is proved by the observation of Féré (*op. cit.* p. 47), that the dynamogenic value is independent of the [actual] calorific value of colours.

With regard to the first meaning, it is to be noticed that the mental attitude in which a colour is described as stimulating is entirely different from that in which strength is ascribed to it, since in the former case, the attribute is the conscious expression of the *subjective* effect on the spectator, while in the latter, the subject means by strength, not a condition or affection of his own self, but a quasi-objective quality of the colour. This implies both a different direction of attention, as well as the transformation of the physiological effect into a semi-objective aspect, a process with which we shall have to deal later. In point of fact, it is relatively rare that the same individual ascribes both attributes to colours in general.

In respect to the second meaning of strength, it must be observed that, although both purity and strength are 'objective' characteristics of colour, there is again, from the subjective point of view, a deep distinction between them. Purity, as pointed out before, is a *relative* epithet, while strength is practically an *absolute* attribute. I mean thereby that calling a colour 'pure' involves an ideal comparison and mental weighing of one colour against a visualised standard of purity, and therewith a cool, critical, almost disinterested frame of mind, which is conspicuously lacking in the attitude in which a colour is called 'strong.' The sudden appearance of a flaming red, or a dazzling blue, is comparable in its effect to an unexpected blow which does not admit of any collected criticism or comparative judgment. Such a colour is 'strong,' because it surprises, shocks, overwhelms, even hurts. The emotional value of a 'strong' colour is sometimes such that a 'pure' colour, compared with it, can hardly be said to have any, and it leaves the susceptible mind in a state of bewilderment, and often in a kind of ecstatic admiration which differs *toto coelo* from the superior, distant, appreciation of 'purity.'

For this same reason, the well-known delight of children and primitives in 'pure,' uncompounded and unbroken colours is, I would suggest, not due to their purity, but to their strength. This would seem much more in accordance with their general mental state than the criterion of purity, which implies a disengagement from, and even a relative insensibility to the immediate powerful appeal of colour, and which is only reached by mental and emotional training. In many cases the habitual use of 'purity' as a criterion may even be regarded as the expression of the growing callousness towards, and an acquired disregard for, colour as such, and not by any means as a special refinement of taste.

5. *Purity* 2. This second kind of purity is very simple, being directly traceable to *saturation*. Any colour, of any tone, may be said to be a 'fine' or 'good' colour on the ground of exhibiting a satisfactory degree of saturation, while colours of manifestly insufficient saturation are called dingy, poor, insipid, indefinite, &c., as possessing not enough 'colour' or 'body.' The purest instance of the application of this criterion is A (Table I), who said that he found any colour pleasing, provided it was sufficiently saturated; B, 1 and 2 are similar, though less consistent, illustrations of the same type. The remark which was made with respect to Purity 1, applies also to this kind of Purity, namely that it is essentially a *relative* aspect, both on the objective and the subjective side. Even pale and broken shades, such as purples or mauves, pinks, light blues and greens may, therefore, be found pleasant on the ground of purity, as showing the best saturation possible for each individual tint: and consequently the use of this aspect does not preclude the acceptableness of shades other than the spectrally saturated colours.

Against the generally accepted belief that saturation represents the prevalent reason for colour-preferences, I would urge, on the ground of these experiments, that this is an aspect of relatively rare occurrence. As I have already remarked in the introductory observations, I believe the method adopted in such experiments as Cohn's to be mainly responsible for the apparent predominance of this aspect.

6. *Weight.* The question of the apparent heaviness or lightness of colour I have discussed, with the help of a special set of experiments, in a previous number of this Journal[1], and I have there shown that it is an aspect of which *luminosity* represents the objective basis. 'Weight' is merely a generic term for very various kinds of impressions, as long as they are due to the notion of some inherent 'moreness' of some colours, compared with others. If we speak of the 'density' or 'thickness' of a colour, or of a sort of 'unyieldingness' or 'impenetrability,' if, on the other hand, we speak of its 'thinness, lightness, spaciousness,' of its 'filmy' or 'diaphanous' nature, or of an

---

[1] 'On the apparent Heaviness of Colours,' *Brit. Journ. of Psychol.*, 1907, vol. II. pp. 111–152.

'oppressive' colour or of one that 'makes us breathe more freely,' in each case we are concerned with this feature of 'weight.' Dark blues, brown, dark greens, dark reds, especially dark purples are such 'heavy' colours, easily oppressive and gloomy, while their pale shades are light and free. Pink is particularly thin, in the sense of being diaphanous pale blue gives the impression of space and infinity (often assisted by the association of the sky), pale green is fresh and cheerful. Pale heliotrope is constitutionally of a too low luminosity to be very cheering, but it exhibits a peculiar delicacy and hazy softness, which contributes considerably to its slightly sentimental character.

As I pointed out in the same place, this aspect is primarily relative. Since, however, the colours in full saturation exhibit differences in luminosity, it is also to some extent applicable absolutely: yellow is the lightest and the weight increases gradually through orange, red, green, to blue, violet and purple as the heaviest.

7. *Brightness.* This aspect, though like weight, the result of luminosity[1], is distinguished from the latter by being, to our perception, *light* instead of *colour*. A very unsaturated colour may retain its pleasingness by reason of its brightness, being perceived as *light*, perhaps faintly and agreeably tinged, whereas considered as a *colour*, it would be rejected as thin, washy and insipid. The appreciation in such cases depends, therefore, very much on the initial adaptation of the subject, according as he is prepared to view the individual colour from the point of view of brightness or of saturation. The principal danger of high degrees of luminosity is that the brightness is felt as 'glaring' or 'dazzling,' frequent complaints of which are to be found in the records among persons of a special type; the corresponding result of very low luminosity with a similar loss of tonality is gloominess, depression and mournfulness. Between these two extremes lie all the variations of brightness, as sombreness, peaceful darkness, bright cheerfulness, &c. With the increase of the tone-element, however, saturation is very apt to make itself felt, as the predominant aspect, and luminosity is accordingly pushed into the background.

We have now individually examined so far the seven aspects of the first and second groups, the 'objective' and the 'physiological' aspects. The distinction between these two aspects will be discussed later.

---

[1] I am quite aware that, as usually employed, 'luminosity' and 'brightness' are synonymous terms for a general feature of colour. As, however, this general feature may give rise to an aesthetic aspect either of brightness or of weight, I have reserved 'luminosity' in order to denote the general feature and 'brightness' to denote the aesthetic aspect.

Group III: The 'Associative' Aspect.

It is an easily verifiable fact that in our recognition of objects their colouring plays only a very secondary part, compared with such features as form, outline or movement; this is owing to the changeableness and frequently arbitrary nature of their colour. In other words, the *cognitive value* of colour is generally recognised to be very low. On the other hand, precisely on account of this very loose connexion between the nature of a thing and its colour, together with the frequency of the same or a similar colour in a multitude of different things, the mental transition from one object to another along the channel of colour-identity or colour-similarity is peculiarly easy and constitutes one of the best-known features of colour generally. That is to say, the *suggestive or associative value* of colour is exceedingly high.

It is, of course, impossible to consider all the various possibilities of associative connexions with colours, even if the interest of the matter would hold out any inducement for such a task. I confine myself, therefore, to a general discussion of the associations found in the concrete cases of my experimental records. Even a classification of these in any but the crudest manner is hardly possible and I have therefore arranged them in the subjoined list rather on a plan suitable to the special interest of this context, than on any more abstract or general principle.

They may be superficially divided into three groups:

A. Natural objects.
B. Artifacts.
C. Isolated and quite accidental associations.

A. *Natural objects.*

1. Sun, sunshine (too common to require mention of special cases).
2. Moon (in various phases and conditions): Nos. 6, 13, 28, 29[1].
3. Sky (in various states), fogs, atmospheric effects.
4. Sea, water, &c.
5. Vegetation: E, Nos. 5, 6, 27, 32, 35.
6. Garden: F.
7. Flowers: Nos. 4, 5. Special flowers: Nos. 5, 6, 10.

---

[1] These numbers and letters refer to the subjects in Tables I and II.

8. Precious stones: Amethyst: F, No. 8, Emerald: No. 7, Opal: No. 6, Turquoise: No. 8. N.B. Mother-of-pearl: No. 6.
9. Blood: F, Nos. 8, 11, 22.

B. *Artifacts.*

1. Textiles (in application and otherwise): F, Nos. 7, 10, 13, 17, 21, 22, 24, 31, 32.
2. Gas-light, Gas-globes, Lamps: Nos. 3, 10, 24, 25, 32.
   Railway-signal: F, Nos. 1, 8, 11, 23.
   Magic-lantern screen: No. 17.
3. Blotting-paper: Nos. 1, 4.
4. Foods: F, Nos. 6, 8, 10, 11, 12, 22, 28, 35.
5. Medicines (once taken): Nos. 6, 12.
6. Private: F, No. 33.
7. Sub-conscious (association felt to be there, but impalpable): No. 7.

N.B. Cases of these last two classes were probably much more numerous, but were presumably in most cases passed by in silence by the subjects, the last (7) possibly not even noticed. The only recorded case (No. 7) is from a subject with exceptionally highly trained powers of introspection.

C. *Isolated and accidental associations.*

1. Crackers: No. 10.
2. Musical note: No. 11.
3. Sand: No. 22.
4. Skating: No. 22.
5. Steel: No. 27.
6. Church: No. 30.

The attitude of aestheticians towards association (in the sense of revived memories of at one time actually perceived objects) has always been very uncertain. Kant's distinction between 'free' and 'dependent' beauty initiated the general depreciation of the aesthetic value of association, with the result that associations have at times been considered as a quite illegitimate and inadmissible factor of aesthetic appreciation. This represents an attitude, prompted by the wish to get rid of a very troublesome subjective source of pleasure and to reach the ideal of an universal validity of aesthetic judgments by the exclusion of a factor which constantly upsets any common consensus. Fechner[1], with his 'associative' as opposed to the 'direct' factor, has the great merit of having opened up again this side of aesthetic enjoyment, although he restricts his considerations mainly to associations of the most general type, such as the connexion of green with verdure and rustic life, or of blue with the sky, the sea and lakes. At the present time the same uncertainty still prevails; in England there has generally been the tendency to make too much capital out of associations and to explain everything by them; on the Continent Lipps[2] rejects all associated contents as extra-aesthetic, while Volkelt[3] takes up a much less rigid attitude towards them. Many aestheticians incline to treat them merely as convenient explanations of some inexplicable and seemingly irrational colour-preferences, and Cohn's[4] summing-up—that the majority of persons prefer saturated colours, but that divergencies from this type are presumably due to associations—is typical in this respect.

There is without doubt much difficulty in stating off-hand to what extent associations may legitimately play a part in aesthetic appreciation, especially of colours. It is certainly obvious that in the case of No. 8 who by certain reds was reminded of blood and consequently strongly objected to the colour, the association can hardly be said to possess any aesthetic value, either positive or negative, but was more of the nature of an accident and was certainly felt to be so by the subject. The same remark would apply, for example, to No. 6 and No. 12 to whom the colour suggested medicines once taken and was therefore distinctly unpleasant. In other cases, on the other hand, the matter stands rather differently. If a colour suggests sunsets, or seasons, as trees in autumn, or moonlight, &c., the association loses to some extent its chance character, inasmuch as the associated content lies really within the reach of everybody's experience. Associations in general might consequently be arranged according to the nature of their contents in a graduated series, extending from such general associated contents as in the latter instances, through a scale of increasingly accidental suggestions to the other extreme of purely personal chance associations, as in the former cases. At the general, impersonal, 'objective' end would be found associations as those mentioned by Fechner: by far the commonest is that of green with vegetation, nature, out-door life; blues and blue-greens suggest most easily the sky, the sea, rivers, lakes, water generally; orange or yellow the sun or the moon (here the shape of the colour-surface becomes most important and the frequent suggestions of this kind in the experiments is undoubtedly due to this factor); much less common are associations of red, orange or yellow with fire. Flowers are, on the other hand, distinctly personal and 'subjective' associations, more particularly if they be special, favourite flowers, which then impart, purely on their own account, a peculiar pleasantness to the colour suggesting them, as with Nos. 5, 6 and 10. On the same plane of subjectivity stand the associations with favourite precious stones as those mentioned under A, 8 in the list of natural objects (page 429).

---

[1] Th. Fechner, *Vorschule der Æsthetik*, Bd. I. S. 182, 183.

[2] Cf. his *Æsthetik*, 1903, Bd. II. S. 28 ff.

[3] J. Volkelt, 'Die Bedeutung d. niederen Empfindungen für die aesthetische Einfühlung' in *Ztschr. für Psychol. u. Physiol. der Sinnesorg.*, 1903, Bd. XXXII. S. 14 ff.; also in *System der Æsthetik*, 1905, Bd. I. *passim*. Cf. P. Stern, 'Einfühlung und Association in der neueren Æsthetik,' *Beiträge zur Æsthetik*, herausg. v. Lipps und Werner, 1898, Bd. V.

[4] J. Cohn, *l.c.*, Bd. XV. S. 286.

A similar gradation can be observed in the second division, the artifacts. Suggestions of various stuffs and materials, even their use or application, as dress-materials, curtains, wall-papers, &c., are relatively common; that a circular patch of colour of special tone and saturation should suggest a gas-globe or a lamp is also fairly natural, though it is nevertheless felt by the subjects as peculiar to themselves. The same applies to the railway-signals of the five subjects, given in the list. Associations of food, especially of jellies of different colours, appear as purely accidental; and still more so 'private' associations (of which, as I said, there were probably more than only one), *i.e.* suggestions of so intimate and personal a nature that the subjects preferred not to specify them in detail.

It is evident that associations of such different character as an 'objective' association, *e.g.* [green: vegetation] and a 'subjective' suggestion, *e.g.* [yellow-green: quinine], or [purple: amethyst] are by no means of the same aesthetic value. And the testimony of the subjects themselves concurs in this view. While in the former case a person might express surprise that someone else should not have been reminded of vegetation by a green, the subject in the latter case was perfectly conscious of the entirely personal and accidental reason for his liking or dislike of a colour, and might even hesitate to give his opinion on a colour which, he felt, was spoilt for him personally by such a chance suggestion.

Still the fact remains that in many cases 'subjective' associations enhanced or destroyed the aesthetic value of a colour. Have we any right to treat these instances as illegitimate from the aesthetic point of view? If so, what is the basis on which we might try to establish the distinction between admissible and inadmissible associations? To exclude *all* associations, on the plea that associations generally render aesthetic effects in which they cooperate purely individual, is untenable as soon as only the personal validity of aesthetic judgments and the non-universality of aesthetic appreciation are upheld. It might be suggested that the distinction between 'objective' and 'subjective' associations could serve the purpose of a distinction. It is a significant fact that in most cases, where association conditioned a *negative* appreciation, the association itself was of a distinctly 'subjective' kind, while 'objective' associations nearly always contributed to the pleasingness of the colours suggesting them; and this observation might be taken as a welcome indication of the legitimacy of the latter, while affording a presumption for the justified exclusion of the former. There is, however, subject E as a warning against such rash generalisations on the ground of the nature of the associative contents. The evidence of this case is unfortunately much weakened by being given in its abridged form. E was very subject to a thoroughly 'objective' association, namely that of vegetation. The numerous greens, yellow- and blue-greens were mainly judged by their correspondence with the greens of nature. This suggestion was so strong and persistent that I finally asked E what his main interest was: 'Botany' was the answer. The associative content was 'objective' enough, but nevertheless possessed in its relation to E an importance and interest of the most personal nature.

Again, an association loses in feeling-tone in proportion to its 'objectivity.' Those associated contents which are accessible to all and most generally suggested by the colours, are also the least capable of effectually enhancing or depreciating the effect of a colour, while a deeply personal suggestion is infinitely more powerful in its pleasing or unpleasant results, merely by reason of the intimacy of its relation to the subject. And since—leaving merely theoretical considerations out of account—there would be no *a priori* reason for regarding as illegitimate any source of intense emotional tone, capable of emphatically enhancing the value of an object, it would seem that those accidental and frequently quite irrational factors of associative colour-appreciation are precisely those which ought to be considered as admissible, merely on the ground of their potency and immediate effectiveness.

Opposed to this view is the more or less general tendency of the subjects to consider 'subjective' associations as in a sense extra-aesthetic, while maintaining the aesthetic value of 'objective' associations. Hence it would appear as if the *contents* of the associations are of *some* value for a distinction between admissible and inadmissible factors of aesthetic appreciation, although it is not the contents *per se* which decide the question. Indeed, the whole problem of association, as a factor in aesthetics, demands the consideration of the *subjective* configuration, *i.e.* the relation of the association to the subject himself and his attitude to it. To this I shall return in the discussion of the 'perceptive types.'

Group IV: The 'Character-Aspect.'

By far the most complex side of colour-appreciation is represented by this group, the 'character-aspect.' Fuller and richer in detail than the 'physiological' aspect, and freer from the irrationalities of association, 'character' is a quite peculiar, and—from the aesthetic point of view—an exceedingly important feature of colour.

By 'character' or 'temperament' of a colour I mean the appearance in a colour or the expression by a colour of what, in the case of a human being, would be called his character, or mood, or temperament; the manifestation of a special, more or less definitely developed personality peculiar to each individual. It is, in one sense, what German aestheticians would call 'Stimmung'; but in another sense of this word, the 'character' has a different connotation: for 'Stimmung' implies other features besides this 'character.' 'Stimmung' is not to the same degree anthropomorphic as the terms here suggested. A 'Mondnacht-stimmung,' for instance, need not by any means imply an anthropomorphic element as sadness or melancholy does, and a colour possessing this 'Stimmung' may do so merely by its suggestive power and by an accidental association. The 'temperament' or 'character' of a colour, on the other hand, is strictly and wholly anthropomorphic; it presents itself with all the reality and objectiveness of the temperament of a real being, and it is precisely this reality and objectiveness which render this aspect of colour so interesting from the psychological and especially from the aesthetic standpoint.

The surprising subtlety of distinctions existing between the temperaments of but slightly different colours and the many-sided richness of these characters is such as to cast occasionally some doubt on the genuineness of this aspect in the mind of persons who themselves are insensible to it. Many are, in fact, inclined to consider it as a kind of mystic nonsense, as imaginative romancing or poetic fancy, or as 'reading things into a colour.' The criticism is as valuable as that of a deaf person on a musical composition. About the perfectly spontaneous and genuine nature of the perception of such temperamental qualities there is no doubt to the individual who is conscious of them himself; a further, objective, proof will be furnished by Table IV, subjoined to this section, which contains the independent observations of several subjects on this point, exhibiting the most surprising similarities between their verdicts; and lastly, the endeavour to explain the origins of these interesting features and their descent from generally admitted and quite common aspects may furnish a final proof of their reality and rationality. I should only like to add that, since the appreciation of this aspect is very highly affective, I may be forgiven for using in their description language which may not be deemed sufficiently 'scientific,' for it is practically impossible to render verbally the impressions of this aspect without recourse to metaphors, analogies and mildly imaginative expressions.

Apart from the obvious differences in *content* between this and the other aspects, there exist certain peculiarities of this colour-aspect in respect to its *formal* side, which distinguish it no less effectively from the other features of colour than its content. It is, in fact, not the least interesting side of this whole character-aspect, especially from the theoretical-aesthetic point of view.

1. The character-aspect differs very markedly from the *objective* aspect in that it possesses an affective tone which is peculiarly lacking in the latter. To give one's opinion on a colour from the objective point of view is, *formally*, not very different from delivering judgment upon a piece of furniture or the workmanship of an engine. The cool, dispassionate attitude, together with the dearth of emotional resonance in the subject and the mildness of his critical satisfaction or displeasure, is perhaps the most distinctive feature of the first group. The case is quite different with the character-aspect. The essentially critical frame of mind is replaced by a peculiarly sympathetic openness. There is much affective participation on the part of the subject in the special qualities of the individual colour, a readiness to sympathise with it, and the result is a strikingly vivid, sometimes almost ecstatic, appreciation. This fact finds its objective expression—insufficient because it shows only numerical, not qualitative differences—in the increased number of positive appreciations, compared with those of persons

appreciating on objective lines: for instance No. 30 (+ = 19), No. 33 (+ = 18), No. 34 (+ = 27) as against No. 1 (+ = 11), No. 2 (+ = 8). Other differences will be mentioned later.

2. The distinction between the character-aspect and the 'physiological' group is not of the same nature. Indeed, the appreciations (both negative and positive, but especially the former) of the 'physiological' group are sometimes most emphatic. This is to be expected, seeing the personal and even bodily effects of the 'physiological' aspect on certain individuals. These effects are, on the contrary, conspicuous by their absence in the character-aspect, and this is the point of difference between these two groups. Occasionally it is a matter of considerable nicety to keep the two aspects apart, mainly owing to deficiencies of nomenclature or the careless use of epithets. This occurs especially easily with the effect described as 'cheerful.' Yellow is usually pronounced 'cheerful,' but what does the subject mean by that? Is *he* cheerful, being cheered by the sight of the colour, or does he mean to say that the *colour itself* is cheerful? Is the cheerfulness a qualification of himself, induced by the colour, or an attribute which he applies to the colour, as he might apply it to a friend of his? In the first case we have to deal with a 'physiological' effect (group I, 2, 6 and 7), in the latter with a character-aspect. As a rule the subjects were very decided on this point; many did not even grasp the meaning of the second half of the question, thereby showing that they evidently lacked an experience which could call a colour itself cheerful; they might even indignantly repudiate any such nonsensical notion as calling a piece of yellow paper cheerful in itself. Others, on the contrary, understood at once what was meant by such an application of the epithet and readily expressed the view that what *they* understood by 'cheerfulness of the yellow' was not an induced cheerfulness of themselves, but the attribute of *the yellow itself* (of course, not of the *paper*). A small number realised the experience of both forms of cheerfulness and thus form a most valuable link for the explanation of the character-group as a whole.

The result of this difference is a special kind of independence of the colour-aspect from the individual, and an objectivity of the content expressed in or by the colour, which differs vastly from the personal, sometimes directly organic, affections produced by the 'physiological' features.

3. This characteristic of the character-aspect leads also to the distinction between it and the *associative aspect*. In the case of

associative features—except in those of the most objective type—even the subjects themselves have the latent feeling that it is they, and only they, who impart its meaning to the colour. Compared to this exclusively subjective significance of colour, the character-aspect exhibits a surprising quasi-objectivity. As I mentioned previously and as will be seen presently, the temperaments attributed to colours by various and perfectly independent observers agree fundamentally to an astonishing degree, in spite of various most interesting divergencies in minor points of richness and elaboration. This in itself is a striking proof of the fact that such temperamental 'meanings' are *not* of associative origin, as has so often been maintained. Thus the same relatively stable objectivity, and a similar kind of independence from the subjective state of the spectator, obtain here as a distinction from the associative aspect, as in the case of the 'physiological' group, though the subjective conditions underlying these two latter aspects are of entirely different nature.

The Characters of Colours. As regards the characters or temperaments of colours in general, it may be roughly stated that there appears to exist a kind of temperamental contrast between *red* and *blue*, or between colours containing red or blue. The character of a red or of a tone tinged with red is usually of a sympathetic, affectionate kind; it appears to come out to you with openness and frankness, while blues are of more reserved, distant, even unaccessible temperament, somewhat like individuals described as 'difficult to know.' This temperament is not by any means repellent; on the contrary it has an attraction of its own, by the promise of more thoughtfulness and greater depth than red in its expansiveness seems to offer. A similar opposition is to be noticed also in other respects: red is by far the most active colour; blue, on the other hand, tends to contemplation and reflexion. Red exhibits degrees of energy which are sometimes almost overwhelming; it was once not inaptly described to me as 'gushing,' whereas in blue there is always some measure of coldness and distant state, which to some persons gives it an almost haughty appearance. While red is impressive by reason of its irresistible strength and power, blue has something monumental in its dignified repose and its peculiar spaciousness.

Yellow has a temperament utterly unlike either red or blue. Its essence is cheerfulness and lightness of heart (about the melancholy of yellow see below). It is almost 'fidgety' compared to the deep elemental restlessness of red, and sparkling with life compared to the contemplative seriousness of blue. There is little of the massive strength of red, though a good deal of its sympathetic brightness, and none of the taciturn reserve of blue. Yellow is happiness, sunshine and fun; it is almost impossible to take it seriously (one subject remarked: 'it makes me laugh'); even when it tries to appear serious and deepens into gold, it retains too much of its joyous radiance to be really grand and majestic. It may be splendid, brilliant, but its splendour is after all merely on its surface and a faint suspicion of shallowness is very apt to linger in the mind.

Combined with red into *orange*, yellow loses some of its light spontaneousness by contact with the greater power of red, but robs the red of a part of its unbridled impetuousness; even the natural frankness of red seems to suffer to some extent by the mixture with yellow. If, on the one hand, this combination produces one of the most magnificently brilliant tones, of almost unrivalled intensity, warmth and life, yet the fusion of temperamental differences—however slight these differences may be—is apt to introduce a note of discord, which certainly makes the colour all the more interesting by increasing the complexity of its temperament, but seems to suggest to many persons a kind of duplicity, even of treachery or stealthiness. This may appear strange in the case of orange but it nevertheless occurs sometimes, as the following table (page 439) will show. This temperamental weakness is particularly marked in compounds with yellow; other combinations tend to produce similar impressions of many-sidedness of character, though not necessarily the effect of unreliability which seems to be inherent in yellow. On the whole, orange is not an 'intimate' colour; its attraction lies more in its magnificence than in its more inward qualities.

What yellow so conspicuously lacks, *green* possesses in an eminent degree, namely solidity of character, sometimes even to the verge of pedantry and the 'bourgeois.' As distinguished from red, its fundamental quality is restfulness; as opposed to blue, it is expansive and jovial instead of cold and reserved. It is essentially healthy, without a shadow of morbidness. At the same time it is one of the most fluctuating and variable colours from the point of view of the character-aspect, which seems to be due to the fact that an absolutely pure green is very rarely met with and that the faintest admixture of yellow or blue influences its tone (or hue) in a much higher degree than that of other colours. The various degrees of luminosity also affect its temperament much more than they do in other tones. In an absolutely pure, fully saturated green, as in the spectrum, there is hardly any trace of pedantry left; even its solidity and placidness are very largely lost in the luminous richness of its tone. Such a green may show a degree of refinement, which is not as a rule characteristic of greens as a class. As a whole they are healthy, expansive, good-natured and very reposeful. Mixed with yellow, however little, green is very apt—unless a low luminosity keeps up its solidity, as in dark olive—to lose its soundness of character and easily appears sickly, unreliable and treacherous. It becomes 'bilious,' irritable and peevish. The addition of some blue, on the other hand—as in emerald-green—at once adds some reserve and refinement to its healthy-mindedness; it checks its joviality, increases its reflectiveness and strengthens its purpose.

One of the most interesting colours from the point of view of character is the group of *purples*, both blue- and red-purples. Their fascination lies largely in the strange combination of the temperamental contrasts peculiar to the components, red and blue. Still more than orange, purple is a self-contradiction, but without the element of duplicity or untrustworthiness inherent in compounds of yellow. This internal antagonism places it in a very marked contrast to green, and purples incline, in opposition to green and its healthy-mindedness, to some degree of morbidness or at least sadness and melancholy. This note is especially pronounced in blue-purples, where the predominance of blue tends to lower the luminosity and to accentuate the element of pensiveness, self-contemplation and severity. If the luminosity is very high, as in pale mauve, the thinness of the tone imparts to the colour a particularly marked morbid sentimentality, though always with the quality of almost fastidious refinement and feminine delicacy. A pale —but not *too* pale—blue-purple is a distinctly 'mystic' colour; a fully saturated blue-purple possesses a degree of stateliness and depth, which, combined with the somewhat abnormal, many-faceted temperament, produces the impression of a mysterious not easily fathomed personality: one of the subjects (No. 35) described a colour of this kind very pertinently as 'a person with a past.' The red-purple variety exhibits the same complexities of character, but the prevalence of the red element gives it considerably more frankness, impulsiveness and sympathy. Red-purple is perhaps, from this point of view, the most fascinating of all colours: especially if fully saturated, there is unlimited strength in it, not the irresistible dash of red, but an energy controlled and spiritualised by the thoughtfulness and sensibility of the blue;

there is all the affectionate impetuosity of the one, coupled with the reserved inwardness of the other.

After this cursory description of the temperamental aspects of the principal colours, as I have come to know them, partly from my own experience and partly from the accounts of others, I add here a special table, abstracted from the records, which shows the various characters ascribed by different subjects to the various colours. The bracketed numbers following the attributes indicate the number of the subject in the main table.

TABLE IV.

Character Analysis.

1. *Good sat. yellow*—gay (34), not aggressive (35).
2. *Yellow-green*—insipid (16).
3. *Green-yellow*—gay (34), sickly, insipid (35), insincere (33), undecided (32), oppressive (29).
4. *Scarlet (orange)*—undecided (35), strong (29), violent (27).
5. *Green-blue*—mild, innocent (35), hopeful, feminine (33), empty, worthless (28), calm, placid (14).
6. *Scarlet (red)*—expansive (34), stealthy, underhand (35), lack of strength, undecided, morbid (33), jovial, good-natured (31), melancholic (29).
7. *Unsat. yellow*—bright, cheerful, spontaneous (35), morose, treacherous (33), serious, sad (28), gentle (16).
8. *Unsat. yellow.*
9. *Dk. blue-red*—sullen, stupid (33), mournful, dead (32), negative qualities (31), dismal (16).
10. *Bright green-yellow*—indistinctive (35), not quite frank (33).
11. *Salmon*—stubborn (35), not quite frank (33), empty, meaningless (28), melancholic, mystic (26).
12. *Very bright yellow*—cheerful (34), bright, cheerful, spontaneous (35).
13. *Red-orange*—cheerful, homely (34), underhand (35), warm, sympathetic (31), violent (27).
14. *Unsat. blue*—simplicity, soothing (35), restful, lacking energy (33), nondescript (31), morbid (29), undecided (27).
15. *Sat. red*—self-sufficient, grand, majestic (34), strong, dashing (35), boisterous (33), cheerful, lively (32), active, sympathetic (31), violent (27).
16. *Sat. yellow*—sympathetic (34), soft, insipid (35), restless (30), laughing (28).
17. *Dull red*—harmony, contentment (34), mysterious, unique (35), morbid (33), cheerful (30).
18. *Unsat. blue*—spacious, mild (35), undecided, vacillating (33), depressing (30), stately, serious (28).
19. *Blue*—expansive, grand, severe (34), no energy, distant, serious, diffident (35), little morbid (33), soft, inspiring (30), soothing, no severity (29).
20. *Blue* (less bright)—insipid (35), undecided (30), shallow (26).
21. *Yellow*—bourgeois, jovial (35), treacherous (33), aggressive, treacherous (30), force (26), sad (22).

22. *Red*—sympathetic, bright, severe (34), self-confident, open (35), vigorous (16), cheerful (31).
23. *Full yellow*—sympathetic (34), cheerful, bright (35), cheerful (30), melancholic (29), energetic (26), flash, shallow, conceit, frank, brilliant, sympathetic (33).
24. *Lilac*—sad (34), potential energy, 'person with a past' (35), morbid, jealous, inoffensive (33), melancholic (31), empty, flippant (28).
25. *Bright green*—pleasant, fearless, reckless (35), flashy, cheery, optimistic (33), treacherous (31), half-hearted (30), serene life (28).
26. *Bright yellow*—bright, mysterious (34), problematic, determined, narrow-minded (35).
27. *Green*—pleasant, fearless, reckless (cf. 25, but less so) (35), treacherous, unstable (33), undecided (30).
28. *Orange*—mysterious, attractive (34), aggressive, undecided (cf. colour 26) (35), gentle, delicate (31), mystic, oriental (26).
29. *Orange*—cf. 26, but less so (35), vanity (33), oppressive (29).
30. *Unsat. blue*—very frank, truthful, not very characteristic (35), phlegmatic (33).
31. *Bright unsat. blue*—fatigued (35), slightly morbid (33).
32. *Bright yellow*—light, playful (34), not very robust, a little slack (35), unnatural (33), cheerful, aggressive (30), melancholic (29).
33. *Unsat. blue*—stubborn force (35), dainty (26).
34. *Unsat. purple*—undecided, deep? (35), soft, reposing (30), slightly mystic (26).
35. *Bright red* (orange)—strength, plenty of life (33), fiery temperament (31).

The interest of this comparative table lies in the fact, that, although the observers were perfectly independent of each other, there is yet on the whole a striking agreement between the temperaments which they severally attribute to the various colours. Certainly there are also differences; and these differences themselves are of the highest interest, partly as showing the extraordinary subtlety and the endless variations of colour-appreciation in respect to this aspect alone, and partly in view of their explanation.

Such differences appear to me to be explicable on the two following grounds:

(1) They represent as a rule no radical differences in the perception of the character-aspect, but merely a superficial, sometimes only verbal divergency. In that case they seem to be due to a special emphasis laid on *one* temperamental feature of a colour rather than on another. For a colour may, and generally does, possess several sides to its character, and the stress laid by two different observers upon such different sides may produce in the character-perception apparent differences, which are, however, perfectly reconcilable in reference to the *fundamental* character of the colour. An instance will illustrate the working of this explanation: Colour 19 is variously described as 'expansive, grand, severe'; 'no energy, diffident, serious, distant'; 'a little morbid'; 'soft but inspiring'; 'soothing, no severity.' 'Grand,'

'severe,' 'serious' and 'distant' are obviously only different ways of saying the same thing, namely expressing the general characteristic of reserve and coldness peculiar to blue; 'no energy' and 'diffident' do at first sight appear as contradictions in the context, but are not really at variance with the previous features; for the same observer (35) ascribed 'energy' generally to *red*, a feature, as explained above, essentially lacking in blue; both 'lack of energy' and 'diffidence' and slight 'morbidness' are really intended to characterise the absence of outward energy and the self-centred contemplativeness of blue—which rather reminds one of Hamlet's temperament: 'The native hue of resolution is sicklied o'er with the pale cast of thought.' Again 'expansiveness' is a misnomer: what the subject, using this expression meant, is, not 'expansiveness' as I applied it to green, for instance, viz., frank, jovial, goodnaturedness, but rather 'spaciousness,' and is intended to express the grandeur of blue. Lastly, 'softness' is manifestly a negative attribute, namely, the absence of active strength or aggressive intensity, and the 'inspiring' quality does not belong to this group at all, being evidently due to association with church-matters, the subject calling it a 'religious colour,' as will be seen in the records (No. 30).

Other instances of similar discrepancies, explicable on precisely the same lines, are fairly numerous and will easily suggest their own solutions; the present one will suffice as a representative case of this type.

(2) On the other hand, there are differences which cannot be accounted for by different formulations of the same fundamental temperament-aspect, as they show wider divergencies, not so obviously reconcilable with each other. Colour 25 is a case in point: it is described as 'fearless, reckless'; as 'flash, cheery, optimistic'; 'treacherous'; 'half-hearted'; 'serene.' Putting aside 'fearless,' 'cheery,' 'optimistic,' 'serene,' 'reckless' and 'flash'—which would fall under the heading just discussed—there is 'treacherous' and 'half-hearted,' which seem to stand in a very marked contradiction to cheeriness and fearlessness. Other illustrations are supplied, for instance, by colours 2, 4, 5, 7, 9, 14, 18, 24 and 33. Differences, like these, I would suggest, are due not to the different emphasis of different temperamental sides of the same character, but to the prominent part given to the interpretation of different *objective* features of the colour. While one subject may lay more stress upon the tone (or hue), another may pay special attention to its saturation or luminosity, with the additional complication that both the latter are very relative notions and that what seems to one observer a fairly saturated colour may appear to another as a rather poor

specimen. Thus in colour 25 'fearlessness' seems to be to the one subject the result of the emphasis being laid upon *saturation*, while 'half-heartedness' or 'treachery' is to others the interpretation of the *tone*-quality. As pointed out before, green is, in its character aspect, very sensitive to the addition of yellow, and under unfavourable conditions of lighting there may easily have appeared to some subjects a tinge of yellow in the green. Colour 33 is a still more decisive case: 'stubborn force' is manifestly due to the tone-element and the low luminosity of the blue; while 'daintiness' is the expression of its slight saturation and lack of 'body.'

One of the most disconcerting statements is the constant verdict of No. 29 on yellow as being 'melancholic.' This extends in his case even to some compounds containing yellow, as scarlet (6) or salmon (11). This melancholy of yellow stands in direct opposition to the general opinion of other observers who all agree that yellow is essentially of a cheerful disposition. One might suspect No. 29 to be prompted by association, were it not that he is relatively free from association in his colour-appreciations and that he himself was certainly unable to point to any suggestion which might explain this melancholic impression of yellow. Though he is the only subject exhibiting this peculiarity among the 43 tested, this idiosyncrasy is perhaps not as rare as this proportion might lead us to suppose. With a certain effort of the imagination I find that to represent to myself yellow as melancholic, sad, desolate, is by no means beyond the limits of possibility, although normally I agree with the majority as to its cheerfulness. On inquiry I found that the sadness of yellow is not as infrequent as I had at first thought. It is also to be noticed that Cohn[1] as a result of his experiments inclined to the conclusion that yellow occupied a special place in respect to its feeling-tone, though Major[2] disputed this and maintained that yellow was just as frequently preferred as other colours. It is, besides, interesting to observe that there is a similar contradiction in the symbolic significance of yellow[3]: besides being the most precious and magnificent colour in the ancient Greek and early Christian rituals, it was also symbolically identified with envy, jealousy and malignity generally. Yellow obviously combines in itself two entirely distinct effects, probably due to two distinct causes. The reason for its cheerful, happy impression is undoubtedly its high luminosity, its brilliancy and the stimulating effect of its tone. But its melancholic effect? Perhaps we have to assume some special physiological action of yellow, which is too weak to be noticed by ordinary individuals and only makes itself felt in specially sensitive subjects. We might see an instance of this in the effects observed by Prof. Latta[1] in a case of successfully operated congenital cataract: 'The evidences of aesthetic appreciation in the case of Carruth are slight. He is fond of lights and bright things generally. The colour red gave him at once a feeling of pleasure; but dark colours did not specially depress him. Yellow, however, had a remarkable effect upon him. The first time he saw yellow he became so sick that he thought he would vomit. This feeling has never recurred. I don't know how this is to be explained, but there is doubtless some physiological reason for it.' It is obviously for the same unknown reason, that No. 29 and so many individuals find yellow melancholic. Possibly such effects of yellow and the consequences of santonine-poisoning[2] are not without some hidden physiological connexion.

---

[1] *Loc. cit.* Bd. x. S. 599.

[2] *Loc. cit.* p. 66.

[3] Th. Voebehr, 'Die Neidfarbe Gelb,' *Ztsch. für Æsthetik*, Bd. I. S. 355–365.

Genealogy of the Character-Aspect.

There are three views at present concerning the origin of these curious temperamental features which by some persons are habitually attributed to colours:

1. Association of actually perceived objects, persons or conditions with a given colour.
2. Association by 'similarity.'
3. A complex line of development mainly from the physiological aspects.

1. The first of these theories may straightway be dismissed as untenable. As mentioned before there exist profound differences between the associative and the character-aspect both on the objective and on the subjective side. If it were merely a question of arbitrary associations (and all such associations are arbitrary and accidental), there would be no explanation for the peculiar constancy with which the same temperamental qualities are attributed to the same colours by different persons. Secondly the characters of the colours would hardly exhibit the strange independence from the subject which in point of

---

[1] R. Latta, 'Notes on a Case of successful operation for congenital cataract in an adult.' *Brit. Journ. of Psychol.*, 1905, vol. I. p. 145.

[2] Helmholtz, *Physiol. Optik*, 2te Aufl. S. 861.

fact they do possess. And thirdly the subjects themselves might be able to give evidence on the point of association, unless we quite gratuitously assumed that in *all* cases the associations responsible for the result were *subconscious*.

As a matter of experience, the attribution of character to colour is certainly not associative, consciously to the subject, in the sense in which the features, quoted under the 'associative' aspect, are associative. In fact, in all cases in which character-attribution and association occur concurrently with the same observer, the observer invariably and definitely was able to draw a distinction between them. Such cases are very rare among subjects habitually indulging in character-attributions: No. 31, colour 6, and No. 33, col. 9, are instances in which the distinction was quite obvious to the subject; No. 4, col. 15, is an illustration of a different process, namely, the suggestion of a moral quality ('brutality') by association; but it must be noticed that the red *suggests* brutality, but that it is not brutal itself, in the same way as col. 9 suggested to No. 33 a 'stupid person,' but the colour itself appeared 'sullen' to him. In this latter case, the temperament of the colour itself acted very probably as a suggestion.—Finally, as will be pointed out presently, associations affect the ultimate appreciations in a very different way from temperamental qualities.

At the same time I am not prepared to rigidly deny that associations may not occasionally contribute to the elaboration and the enrichment of the characters of colours. This may occur in particular with general associations, not sufficiently strongly personal to destroy the objectivity of the character-quality. Thus the association of vegetation, nature, out-door life may conceivably contribute to the general healthiness of green, as that of the sky may make a pale blue colder, more spacious and more diaphanous. But on the whole little seems to be added to the temperaments by associations; and as regards the temperaments themselves I am of opinion that they are not due to any such associative processes.

2. The second view of an origin by 'association by similarity' is in particular represented by Lipps, and accepted, side by side with other hypotheses, by Volkelt[1]. It is called by the latter 'purely optical empathy[2]' and the former terms it more explicitly 'association by

---

[1] J. Volkelt, *System der Aesthetik*, München, 1905, Bd. I. S. 268. Cf. also P. Stern, *l. c.*, S. 58 ff.

[2] The term 'empathy' has been suggested by Prof. J. Ward as translation for 'Einfühlung' and will be used as such hereafter.

similarity of the psychical movement[1].' He explains it, for instance, in the following passage[2] in this manner, referring to the well-known fact that a deep blue appears not infrequently comparable to the vowel u: 'this cannot mean that in the colour or the sound, as regards their sensational content, lies any common element. The contents are of necessity incomparable. Colours possess no auditive and sounds no visible qualities.... This similarity must therefore be sought for elsewhere.... It exists primarily between these *two experiences*, a deep colour and a deep sound, *i.e.* in the agreement as regards the manner in which they affect me, namely, peculiarly quietly, seriously, broadly, etc.... But this similarity between my experience of a deep colour and that of a deep sound must have its reason; and this reason can only lie in a similarity between the actual sound- and colour-experiences; *i.e.* there must be an agreement between the *manner* or *form* of the experiences' (not between their respective *contents*). Thus it would be possible to receive a character-impression from a colour without any intermediate links of either an associative or 'physiological' nature, exclusively on the basis of the similarity which we may feel to exist between the manner in which we are affected by a colour and the manner in which we are affected by some particular emotional state. The point of comparison would lie, not between the perceptive contents 'red' and 'energy,' but between the similar modes of our being affected by red and by the manifestation of energy. Stern endeavours to illustrate the point by the similarity between a musical sequence in a certain key and the same sequence transposed into another key. Though not a single same note may occur in both, yet we recognise the sequence as the same on the ground of their likeness in *form*. In the same way, we may, on the basis of such an association by similarity, connect two experiences of similar 'form' or movement but of completely different contents, *e.g.* the optical image of a colour and the impression of a definite character or temperament. The same kind of connexion, it would seem, is to be found in 'coloured hearing,' in the occasional identification of certain colours with certain persons or certain days in the week, and in the transposition of melodies into linear forms or of linear forms into movements.

---

[1] 'Les rapports d'analogie' of P. Souriau, *L'Imagination de l'Artiste*, Paris, 1901, pp. 99–101.

[2] Th. Lipps, *Grundlegung der Aesthetik*, Leipzig u. Hamburg, 1903, Bd. I. S. 426, 427; 'Aesthetische Einfühlung,' *Ztschr. für Psychol. u. Physiolog. der Sinnesorg.*, Bd. XXII. S. 441–446.

The theory thus appears most fruitful in its many-sided applications and may well be accepted in the absence of definite data making any other view more probable. I confess, however, that it appears to me rather as a make-shift hypothesis, mainly on account of the undemonstrable assumptions which it involves.

3. Lipps seems, in this theory of 'association by similarity,' actuated primarily by his radical objection against 'empirical' associations and against 'organic sensations' within Aesthetics. Volkelt, without this particular bias, seems to me to do more justice to the diversity and complexity of aesthetic and especially colour-experiences by his treatment of the subject; and I am glad to notice the points of contact which happened to appear between his speculations, based mainly on personal experiences and observations, and the conclusions suggested to me by my experiments in the matter of colour-effects. Volkelt[1] while accepting the theory of the 'purely optical empathy' admits besides not only an associative link, but also physiological links (organic sensations) between the optical impression of a colour and its temperamental significance—thus allowing not only for the diversities of colour aspects, but also for the possibilities of individual differences in the perceptive processes. The above experiments led to very similar results, corroborating in many points Volkelt's hypotheses. I am even inclined to believe that the 'purely optical empathy' is a superfluous theory, as the actual facts appear to me explicable on a mainly physiological basis.

I would submit then, that a development from the 'physiological' aspect in certain directions would suffice to explain the formation of these temperaments or characters somewhat in the following manner:

The first requirement is the assumption of a change of mental attitude on the part of the subject to make any such development from the 'physiological' to the character-aspect possible. This change would mean primarily a change in the direction of the attention from the subject's own self to the colour, resulting in what might be called the *exteriorisation* of the aspects of strength, stimulation, temperature, weight, &c. in all their modifications, and their *objectivation* in the colour itself. In other words, the consequence of this alteration of the mental attitude is, that the subject, instead of being conscious of these affections, *as* affections of *himself*, is conscious only of their source as physical attributes—if I may use 'physical' on the analogy of the human being in distinction to 'psychical' or 'moral'—of the *colour*. This is the peculiarity which I pointed out before as a distinction between the 'physiological' and the character-aspect, and described as the quasi-objectivity of the temperamental attributes.

---

[1] J. Volkelt, 'Zur Psychologie der aesthetischen Beseelung,' *Ztschr. für. Philos. u. philosoph. Kritik*, Bd. XIII. S. 174 ff.; 'Die Bedeutung der niederen Empfindungen für die aesth. Einfühlung,' *Ztschr. für Psychol. u. Physiol. der Sinnesorgane*, Bd. XXXII. S. 15, 16; esp. *System der Æsthetik*, 1905, Bd. I. S. 260 ff.

The next step, completing the process, would be a *translation of such physical qualities* as warmth, heaviness, strength, &c. into *psychical features* of the colour. It is—as pointed out above—the differences of such an interpretation which produce in part the divergencies in the impressions of different subjects. This translation of physical into psychical attributes of their possessor is, of course, only an instance of the general well-known anthropomorphic tendency of the aesthetic attitude, and forms part of 'empathy' which has recently been the subject of so much discussion and speculation in Aesthetics. Being a problem of general Aesthetics it requires no detailed reference here, beyond the remark that here the problem of these experiments joins the wider issues of Aesthetics as a whole. With the completion of this step the colour is finally invested with the sometimes so surprisingly complicated characters which we meet with in actual experience.

The process is perhaps best illustrated by a concrete example:

Taking a well-saturated scarlet, the stimulating effect of the scarlet, instead of being felt by the subject as stimulative, is exteriorised and objectivated in the colour and is perceived as *its* strength. This merely 'physical' attribute is then interpreted as a psychical or moral aspect of the colour and appears as activeness, vitality or energy. Combining these with the temperature-impression, namely its considerable warmth, translated into affectionate openness and sympathy, and with the high luminosity, producing lightness, or in psychical terms, cheerfulness, we obtain as the grand total of the temperament of scarlet, as we saw already previously: its dash and impulsiveness, its loving frankness, its sympathy and joy of living.

Or, to give a more complex case, we might build up in the same way the subtler temperament of a red-purple. The first impression is its internal contradiction: red and blue, producing the effect of a 'complicated' character: there is the red vigour, checked by the thoughtfulness of the blue. The slight tendency to morbidness, due to the relatively low luminosity, still further enhances the complication in contrast with the predominantly red tone of the colour. Yet it is not the pedantic heaviness of a green owing to the energy of the red and the subtler depth of the blue. The temperature is fairly high: there is much of the sympathy and tenderness of the red, underlying the somewhat distant severity of the blue. In fact we continually move in antitheses: cheerfulness and seriousness, sadness and energy, affection and proud aloofness, solidity and refinement, delicacy and richness, in short the many-sided, often internally conflicting, reflective and loveable individuality which a red-purple actually presents.

B. *The 'Perceptive Types.'*

As pointed out in the introductory remarks, it would be conceivable ~~that~~ the same colour might present simultaneously several of its aspects to the same observer, with the result that a kind of perceptive competition between them developed, producing indifference, or fluctuations of the appreciation wavering between them. This is a condition which —though it does occasionally obtain—is very rare owing to the fact that the various aspects are largely exclusive of each other. This mutual exclusion is based upon the subjective configurations implied in each group of aspects. The 'physiological' and the character aspect occupy, indeed, also objectively, a privileged position, inasmuch as the latter is a development of the former and the former consequently works in the same direction as the latter; but even here the subjective conditions are sufficiently different to prevent any competition between these two aspects—*a fortiori* is this the case with aspects so radically different as the 'physiological' and the 'objective' or the 'objective' and the 'associative.' Now, considering the matter entirely from the point of view of the *subjective* conditions, instead of from the point of view of the colour and its aspects, we find what I propose to call '*perceptive types*' among the subjects.

A 'perceptive type' is the collective term for a number of individuals who tend to the perception of one aspect, or group of aspects, rather than of another. Thus the relatively exclusive emphasis of some one particular aspect becomes typical of this special group of persons and appears to represent their (more or less) habitual and fixed mode of behaviour[1].

---

[1] The question as to how far the same individual shows any constancy in his adherence to one particular type is a matter for further experiment. But whatever the results may be, they would affect only this special point, without invalidating the existence or characteristics of these types in the abstract.

These 'perceptive types' correspond to the main groups of colour aspects, and we can consequently distinguish four main types:

I. the '*objective*' type,
II. the '*physiological*' type,
III. the '*associative*' type,
IV. the '*character*'-type.

It must, of course, be borne in mind that this is a purely theoretical scheme which only in its main lines is applicable in practice; in other words, that in actuality it is always a question of 'more or less' and that rigid lines of division are a practical impossibility. Thus absolutely pure representatives of these types hardly ever occur and the majority of individuals shows admixture of other types, although there is nevertheless generally a distinct tendency to a more consistent adherence to some one type. It will presently be seen why transitional cases are to be expected and in which direction deviations from the original type usually occur.

In the records to the experiments, I have arranged the subjects in groups, each of which represents one of the four types; thus,

I. the '*objective*' type is represented by A, B, Nos. 1 and 2;
II. the '*physiological*' type by C, D, Nos. 13—25;
III. the '*associative*' type by E, F, and Nos. 3—12; and
IV. the '*character*'-type by G, H, and Nos. 26—35.

The purest instances of each type are: A for I, Nos. 22 and especially 25 for II, Nos. 4 and 5 for III and H and No. 35 for IV; the remainder all show, some only very slightly, the admixture of other types. There are numerous illustrations of the transition from the 'physiological' to the 'character'-type (Nos. 26, 27, 30, 31, 32), *i.e.*, cases of 'physiological' and character-appreciation intermixed, and sometimes even side by side. It is on account of this fact, and because of the genealogical connexion between these two aspects, that I place the 'physiological' next to the 'character'-type in the table.

I will now briefly summarise the leading characteristics of each type, gathering up the various remarks incidentally made earlier upon the subjective side of the appreciation of the different aspects and adding a few notes of interest in this connexion.

1. *The 'objective' type.* Pure cases are exceedingly rare; A is the only one that I met with among the 43 subjects and for him no aspect except the 'objective' one practically existed. B appears to be a fairly consistent illustration of this type, despite the fact that he laboured under 'material complications,' for he always tended to picture to

himself the colours in some particular material and to appreciate them, not so much in themselves, as in use or in application to some definite purpose, *e.g.* wall-papers, curtains, &c. No. 1 shows three instances of association, and No. 2 eight instances of 'physiological' (temperature-) impressions.

The criteria of this type—and the aspects most emphasised in their appreciations—are Purity 1, Purity 2 and Brightness (Aspects: Groups I and II, subd. 1, 5 and 7 (page 418). Scattered instances of the typical behaviour of this group of subjects are fairly frequent among other types, especially in cases where the characteristic criteria of other types are, for some reason or other, inapplicable.

Perhaps the most striking, general peculiarity of this type is its constant tendency to a more *purely intellectual*, than emotional appreciation. This strong intellectual element is, as I pointed out before (page 421), characteristic of the aspect of Purity, either of tone or saturation, and betrays itself, first in the obvious mental comparison involved in the attribute 'pure,' secondly in the marked inclination to analyse mixed colours into their components, and lastly in the general somewhat dogmatic standardising of the appreciations. The 'objective' is in a sense by far the most critical of all attitudes, but also the least appreciative, as is shown by the relatively large number of rejections (Nos. 1, 21, No. 2, 24); but it is by no means the most sensitive. That it must needs be sensitive, because it is 'objective,' appears to me as a mere popular prejudice in favour of objective attitudes in general. Members of this type give, in fact, generally the impression that they cannot get into any kind of intimate relation to colour, and that, in the absence of a more personal sympathy, they take refuge in a distant, almost hostile position. As far as my experience goes, this seems to correspond to the actual facts: A said that he had hardly any sympathy for colour and was practically insensible to colour-effects in ordinary life. I have similarly reason to doubt B's susceptibilities to colour. Nos. 1 and 2 were not personally known to me, but from my notes I see that No. 1 according to his statement hardly ever appreciates colour *per se*, though he prefers green for practical purposes, *e.g.* for decorations. No. 2 prefers blues and dislikes red, which points—as will be seen presently—to his belonging in part to the 'physiological' type, which the records themselves render quite probable.

On the whole, it can be said that the absence of any definite colour-preferences in the abstract is usually an indication that the subject belongs to the 'objective' type. As he is prepared to like any colour—provided that it comes up to his standard of tone-quality, saturation and luminosity, and is, in fact, a 'good colour'—abstract preferences are hardly to be expected, though the subject may have more pronounced leanings towards particular colours for special purposes. But the consideration of such special purposes introduces side factors, such as usefulness, appropriateness, &c., which in themselves are not criteria involved in the appreciation of colour *per se*.

Lastly, it may be noticed that appreciations of this type appear to be special cases of a large class of aesthetic appreciations based upon what might be called 'conformity to type.' Appreciations of this kind are intrinsically comparative; they rely upon the relative approximation of the object to some type or standard which the subject has set up for this particular class of thing, and its acceptance or rejection is conditioned by the comparative conformity to or deviation from this type. Such appreciations occur frequently enough, not only in the case of colours, but also with linear complexes, in architecture, in painting, statuary, and in fact with any kind of aesthetic object. That they are so especially common in the case of unusual and strange objects with which—to use a common expression—'we don't quite know what to do,' *i.e.* with which we cannot, owing to their strangeness, get on to any more personal footing, reveals their nature in its purest form. For they are essentially *external* appreciations, devoid of any implication of our inner self, impersonal, impartial, unemotional, in short, 'objective.' And in so far as such judgments do not involve the appreciation of the object in itself and in its unique individuality, but only in its relation and in comparison to the average type of that kind of object, they represent, I venture to think, the crudest form of aesthetic appreciation in general; they fail both objectively and subjectively in the peculiarly 'autotelic,' individualising, isolating character of aesthetic appreciations and are generally expressive of the inability of the subject to place himself in any closer, more inward, and more personally interested relation to the object.

2. The *'physiological'* type is very largely represented in the records. This may be accident, but more probably it lies in the nature of the type itself. There are three reasons which can, in my opinion, be made responsible for this predominance. The first is that presumably the attitude, characteristic of this type, is the most obvious and natural of all. Susceptibility to the 'physiological' effects of colour is very likely the normal form of reaction to colour-impressions, which in a type like the 'objective' has been only artificially superseded, and

which requires special imaginative and emotional powers for its development into the 'character'-type. The second reason which in the case of these experiments can be adduced for explanation, is the large percentage of natural science-students who took part in them. It is significant that with few exceptions, genuine members of this type were men engaged in the pursuit of objective sciences and, if this is really a valid reason for the large share of cases claimed by the 'physiological' type, it represents an interesting instance of the influence which training and habitual preoccupations—quite undoubtedly—exercise upon the casting of the mentality and thereby upon colour- and generally aesthetic appreciation. Darwin's experience will always remain a classic illustration of this influence. Lastly, I think that the defects of the material and the disadvantages of the experimental conditions are a third, not unimportant factor, making for the predominance of the 'physiological' type. For I believe that this group is in reality much smaller than would appear from the records, inasmuch as, under better conditions, a fairly large number of its members would have proved to belong either to the 'character'-type or to the transitional cases between the 'physiological' and the 'character'-types. The ground for this belief is that I know certain subjects (notably Nos. 13, 23 and 24) to be much more highly appreciative of colour in ordinary life than they appear to be as members of *this* type. The hypothesis treating the character-aspect as a higher development of the 'physiological' aspect, would easily explain such cases as instances of an imperfectly developed attitude, stunted by the artificially strained and generally disturbing condition of the experiments. No. 24 gave it as his opinion that the conditions actually interfered with his appreciation. Very probably this was the case in other subjects too, though it is only reasonable to assume different degrees of sensitiveness in this direction among different subjects.

The criteria of this type are the stimulation- and the temperature-aspects of colour and very frequently its brightness. Complaints of the latter (the colour is 'glaring,' 'trying to the eyes,' 'fatiguing to look at') are always indicative of the 'physiological' type. Stimulation- and temperature-effects being the specific properties of the red-yellow series on the positive, and of the green-blue series on the negative side, it is especially the colours of these two series which are accepted or disliked by members of this type. The preferences are in the last resort decided by the general constitution of the individual, accordingly as he likes being stimulated or soothed, or prefers warmth or coolness.

Thus also an *abstract* preference for either red or green points nearly always to the 'physiological' type. Some even prefer reds *and* greens, or reds *and* blues. As regards temperature-impressions, No. 22 is the purest case that has come to my notice,—the subject, as he told me afterwards, actually shivered, when looking at a particularly 'cold' blue. Subject C was also powerfully influenced by his abhorrence of cold in his appreciations; to him the *ne plus ultra* of perfection is represented by orange as the combination of the warmest shades, red and yellow. In the case of colours where the 'physiological' criteria are not immediately and obviously applicable, the subjects of this type have generally recourse to 'objective' criteria, such as purity in particular, unless they leave the question of liking or dislike open, to which, however, few as a rule can make up their minds. This explains the frequent occurrence of 'objective' appreciations in this class, together with occasional associations, which are apt to appear to some extent among all types.

Owing to the direct, personal, even organic effects of colour, the appreciations, especially on the negative side, of this type, are as emphatic as those of the 'objective' type are cool and dispassionate. Nevertheless the *positive* appreciations are relatively much more frequent with subjects of the 'physiological' type than among members of the 'objective' class. As against 11 and 8 of No. 1 and 2, the number of acceptances never drops below 13 (Nos. 16, 20, 23) and reaches even 30 (No. 17). With respect to the aesthetic value of this type, I shall presently have to add a few remarks.

3. The '*associative*' type is hardly as large as the 'physiological'; this may again have been accidental, just as it is mere coincidence that of three women examined, two certainly and probably all three belong to this type (Nos. 4, 5, 6). As will be seen from the records, it hardly ever occurs in absolute purity; as a rule it is to some extent interspersed with 'objective' and 'physiological' appreciations, especially whenever associations fail to appear.

It seems hardly necessary to enlarge upon the characteristics of this class, as the name and the remarks previously made upon the associative aspect sufficiently explain the peculiarities of appreciations based upon association. I must only mention two or three points of special interest.

First, with respect to the influence which associations exercise upon the appreciations, it is to be noticed that unpleasant associated contents spoil the colour for the subject, as pleasant associations render the colour itself pleasant. This is, of course, by no means surprising, but it becomes of interest, when compared in this respect with the behaviour of the 'character'-type. Only in cases of strictly personal associations did the subjects hesitate to allow any weight to the chance suggestions of the colours. If the association worked, on the other hand, in the direction of pleasantness, the decision was generally left unquestioningly in the hands of the association. Occasionally the anticipated antagonism between an 'objective' and 'associative' aspect could be observed. No. 10 (colours 22 and 32) illustrates this point: both colours, unpleasant 'objectively,' became pleasant through the association, in the one case, of the sky, in the other, of lemon (fruit).

Secondly, regarding abstract preferences, the 'associative' type is most irregular, being at the mercy of the associations. The *existence* of abstract predilections, however, is constant: greens, such as are found in nature (3), pink (4), fawn, buff (5), purple (8), greens (9 and 12), blues (10), even black (11) are some of the predilections of members of this type, partly on account of their associative imports, partly for reasons of use or application.

Lastly, this is also the place for attempting the solution of the question, raised earlier, as to the aesthetic value of associations. I pointed out that, though the *kind* of content associated with the colour does seem to play some part in the problem, the nature of the content *per se* is apparently not the decisive factor. I referred to the subjective configuration and the attitude of the subject to the associated content as a second, no less important factor; and I would now submit that the decision of legitimacy or illegitimacy of association seems to me to rest on the *degree of fusion which can be reached by the individual between the colour-impression and the associated content.* It is one of the many aesthetic problems which cannot be settled by a simple yes or no, but requires the consideration of several points and a verdict in each case on its own merits.

In the case of a purely 'subjective' connexion (as [red: railway-signal] No. 8, col. 8) there can always be noticed a strong tendency on the part of the associated content to keep itself distinct from the colour-impression and to carry its own emotional tone separately from the feeling-tone of the colour.

The more 'subjective' and the more personally tinged, and consequently the more emotionally potent the suggested content is, the more obstinately does it refuse to become subordinate to the colour-impression. This may go so far that the colour almost completely loses its already endangered position in the field of consciousness and becomes a merely inducing factor to the subject's representation. The consciousness of this cleavage and the expression of this dualism of colour and associated content is the unwillingness of a person under these conditions to commit himself to any judgment upon the colour, which eludes him behind the associated content and cannot compete in its feeling-tone with the overshadowing emotional value of the latter. With the increasing 'objectiveness' of the associated content, however, this perceptive competition tends to decrease; in proportion to the generality of the association and the concomitant lowering of its emotional efficacy, the antagonism between colour and suggestion within consciousness disappears and, in the case of a connexion as [green: nature], an actual fusion may be effected, so completely that the colour appears as the direct expression of the association, or, for that matter, the association as the expression of the colour. From this point of view, *i.e.*, the point of view of the associated content, it is consequently not so much a question of the *nature* of the content, as of the possibility, dependent indeed upon the content, of subordinating the content to the colour-impression.

But the instance of E, where in spite of the objectiveness of the content, the fusion failed, indicates a second factor in the process, namely the varying power of the *subject* to *force* the association into subordination to the colour-impression. This power is to some extent independent of the inherent 'fusibility' of the content and represents in its different degrees a second scale of possibilities. It would seem to me to consist both in the effective control of attention and in an especially highly developed 'emotional imagination,' if I may use the expression, *i.e.*, in the capability of reviving emotional states independently of the memory of the particular object or conditions, which gave rise to them, and of bringing them thereby into a more intimate relation to other objects (in this instance to colours) which at first quite loosely suggested them. Putting the process crudely, a person, placed before a colour that merely *suggested* at first a purely 'subjective' content, might, while keeping the colour attentively in the focus of consciousness, detach the emotional tone of the content from its pristine object; and then, fusing the *emotional* memory with the present colour-impression, invest it with a suffused emotional atmosphere, which, in spite of its associative origin, would appear again as the immediate expression of the colour or *vice versâ*. Thus, under favourable *subjective* conditions, as perfect a subordination might be reached with purely 'subjective' contents as is so frequently realised with purely 'objective' associations. In fact, the natural 'fusibility' of 'objective' contents can be said in a sense to consist precisely in the loose connexion between the emotional tone and the object or state to which it was formerly attached; the frequent occurrence of a colour under different aspects and conditions makes it relatively easy to detach its emotional atmosphere *per se* and thus bring the latter into contact with other impressions.

Considering these two intercrossing series of conditions, the 'fusibility' of the content and the 'fusion-power' of the subject, it is obvious that no hard and fast rule can be laid down concerning the aesthetic value of association in general. Provided the fusion succeeds, I can see no reason for denying this aesthetic value. It undoubtedly may help to impart significance and life to the colour, without endangering—thanks to the fusion—the self-contained character of the experience. That it should introduce divergencies of appreciation seems to me no argument against association. Aesthetically, only such associations are illegitimate as withdraw from the colour its emotional tone and appropriate the whole attention and concentration of the subject for themselves, thereby either destroying the autotelicity of his attitude, or even substituting themselves as a kind of aesthetic object. It is this tendency which places purely 'subjective' associations, *as a rule*, outside aesthetic appreciation, though even here the subjective factor may override the disadvantages inherent in the nature of the content.

4. *The 'character'-type.* The apparent relative smallness of this group has already been implicitly referred to and explained in the remarks on the seeming predominance of the 'physiological' type. It was, no doubt, largely due to the unwonted condition of the experiments that among the cases 26—35 only three are practically pure representatives of the 'character'-type, while the remainder are either mixed or imperfectly developed instances. As I suggested above, probably a certain number of the subjects, now classed as 'physiological,' would under less artificial, and in other respects more favourable, conditions have proved to belong to the 'character' group, thus bringing the percentage of these cases more into line with that of other types.

The psychological mechanism implied in the 'character'-impression of this type has already been discussed (page 446) and there is no need to refer again to the suggested explanation of the subjective side of this group. I should only like to point out certain peculiarities of this type, as being of considerable interest in this context.

One of the characteristics of subjects of this class is the absence of any abstract predilections for certain colours. The actual appreciations are catholic to such an extent that it becomes practically impossible for an individual to single out any particular colour for special preference. A person of this type may no doubt have certain leanings towards certain colours for definite purposes, but in the abstract there is usually a latent feeling that *all* colours may be beautiful, each in its own way and perfection, since each one represents an individuality so marked as to render any conscious comparison, such as is of necessity involved in preferences, a matter of practical impossibility. The absence of abstract preferences indicates therefore generally that the individual belongs to the 'character'-type, unless he falls into the 'objective' class, which as I said is also distinguished by its lack of abstract predilections, without however possessing in practice the latitude and sympathy of appreciation of the 'character'-type.

Secondly, an interesting point is to be noticed in the effect which the temperamental qualities of colour have—or rather do *not* have—on the appreciation. As noted above, the appreciations based upon association show the feature that an unpleasant associative content 'spoils' the colour, *i.e.* produces a negative appreciation. It is an important distinction between associative and character-impressions that, in the latter, an unpleasant temperamental expression does *not* necessarily have the same effect. Even a colour possessing a disagreeable character may still be found 'pleasant'—if we apply this rather weak term at all in the case of character-aspects. Individuals vary naturally very considerably in this respect; it is a rule which only holds for the most pronounced cases, for whom indeed there are really no unpleasant colours, except colours which are quite devoid of individuality, *i.e.* very thin, watery, insipid colours; and these are said to be dull, boring, uninteresting rather than 'unpleasant.'

Lastly, a very interesting feature of this group, connected with the observation just recorded, attaches to the *quality* of their appreciations. The whole process of the appreciations, as explained earlier (page 435), shows the strange combination of *personal* elements (the 'physiological' conditions from which the 'character' is developed) with *impersonal objective* elements, as implied in the 'exteriorisation' and 'objectivation' of these physiological features in the colour. This peculiar doubleness in the content of the temperament, with its strange correspondences and interpenetration of subjective states with the objective colour, the fact that—so to speak—the individual projects himself into the object and rediscovers himself in it, will help to explain the seemingly paradoxical statement that appreciations of this type are distinguished at the same time by a maximum of emphasis and by a minimum of *personal* feeling. It is the same paradox which Kant expressed by his famous 'disinterested pleasure,' a contradiction in terms, which yet is after all one of the best short descriptive formulas of the curious mixture of impersonal objectivity and intense personal participation, characteristic of the aesthetic attitude in general. The unity of the attitude is guaranteed by the perfectly central position of the object within the subject's consciousness, and produces states of absorption and self-forgetfulness such as are never found in the frame of mind of other types. This neglect of the momentary self finds an interesting expression in the subject's attitude to the 'aesthetic judgment.' It shows itself in the constantly decreasing feeling of the necessity of a 'judgment,' of a conscious summing-up of the subject's *personal* impressions. This feeling may grow weaker and weaker and occasionally disappears altogether. This occurred notably in the case of No. 35, who became absorbed in the appreciation of the colours in their constantly varied individualities to such an extent, that to be asked for his 'judgment,' almost unpleasantly reminded him of himself, as of a thing quite outside his momentary consciousness, and that he was frequently unable to say whether he 'liked' the colour or not, in spite of the unquestionable fascination and interest which it had for him. The same observation I made also in other cases of the same kind, but unfortunately I did not early enough become aware of the wrench which this sudden 'unclutching' of the subjective machinery meant at the moment to the subjects; with the result that they generally *did* give a judgment—but *after* the clutch was out.

This evident disparity between a definitely formulated judgment and the appreciative attitude of individuals of this type, and the obvious unimportance of such a conscious summing-up—especially side by side with the surprising readiness to deliver a verdict, shown by manifestly inferior types, the 'physiological' for instance—throws an interesting light upon the nature of aesthetic judgments in general, and upon the mistaken emphasis laid upon preference-judgments by hedonistic Aesthetics and its 'pleasure-theories' in particular. There is undoubtedly some serious misconception in a theory built entirely or even mainly upon preference-judgments; for such judgments are more readily and more emphatically given as the results of experiences which—compared with really deep aesthetic appreciations—do not require any degree of absorption, concentration or interest. The fact is that we more easily *judge 'agreeable'* than *'beautiful'* things; and a large part of the experiences dealt with by hedonistic Aesthetics seems to me to belong rather to the former than to the latter kind.

IV. CONCLUSION.

The distinction between the 'agreeable' and the 'beautiful' is, as I suggested in the introductory remarks, a crux in Aesthetics which, though it cannot be solved on the basis of 'pleasure' exclusively, can nevertheless be settled by psychological considerations. It is, in fact, not a question of *quantitative* differences of the *pleasure* that we may derive from an agreeable or beautiful object. A beautiful thing does not so much give us *more* pleasure than an agreeable thing—it might conceivably give us even less—but affects us *differently* in quite a general way.

I should not have raised the question here at all, but that it is of special interest in this connexion; for the problem of a distinction between 'agreeable' and 'beautiful' implies in this context the problem of the *aesthetic values* of the 'perceptive types' which I have attempted to establish.

Appealing to our experience, we find that in some cases we may not be acutely conscious of any such qualitative differences between what we call agreeable and beautiful. It must be even admitted that a thing may be *both* agreeable *and* beautiful, and it is probable that every thing of beauty may appeal to us, side by side with its beauty, also by merely agreeable features. But by comparing two instances, each of a fully developed and pronounced type, one of the agreeable and the other of beauty, we cannot help having the direct and unambiguous experience of the agreeable as something radically different from the beautiful and *vice versâ*.

The difference does not, of course, lie primarily in the object. It is true that, especially by aestheticians, certain *things* have been *a priori* excluded from the beautiful. I mentioned (page 408) this exclusion of single colours by the formalists; the so-called 'lower senses' and objects accessible to us mainly through them, as the pleasures of eating and drinking, scents, states of physical well-being and other organic sensations, &c., have been similarly ostracised. No doubt, there were reasons for so doing, and the main reason why, as a rule, such objects as food or drink, for instance, are said to be merely agreeable seems to be that it is not so much the things themselves which are devoid of beauty, but that it is owing to their accessibility through the 'lower senses' only that they forfeit their claim to being called 'beautiful.' Not that the 'lower senses' *quâ* 'lower' cannot, so to speak, transmit beauty; but that the inherent qualities of 'lower sensations' render it exceedingly difficult for the subject to reach, and to maintain himself at, the attitude in which we ascribe beauty to a thing (the 'aesthetic state'). Such objects as scents, tastes, &c., are therefore not in themselves unbeautiful, merely agreeable, but are excluded from the aesthetic sphere rather by the defects of their channels of transmission than on their own demerits. As a matter of fact, the same thing may at different times appear beautiful or merely agreeable to the same person, and that even with works of fine art, *i.e.* with objects which can unquestionably be called 'beautiful.'

The real difference lies in the *subject.* Even in our language we roughly indicate this difference in the psychical state, when we say: this thing is agreeable *to me,* and this thing is beautiful *in 'itself.'* We thereby express the fact that agreeableness is the consciousness of *our* being pleasantly affected by a thing, whereas beauty appears to the unsophisticated mind—and even to the sophisticated one, while the impression lasts—as an attribute *of the thing itself.*

That *we* are affected in both cases and that both agreeableness and beauty involve a relation between our *self* and the thing is, of course, undeniable; but the point of difference lies in *the different emphasis* which we give to either term of the relation, either to the self or to the thing. This amounts to saying that the distinction consists in the difference of position which the object occupies in our consciousness. If we call it 'agreeable,' *we* occupy the centre; the pleasantness, *our* pleasure is the *rationale* of the whole situation. The thing itself lingers somewhere on the confines of our consciousness and may even fade into a merely inducing factor, a mere source of pleasure. Thus a warm bath after exercise: the bath itself is not the point of stress in my consciousness, but *I myself* with all the delightful sensations of coolness, of softness and of gentle relaxation, and *my pleasure, not identified with,* but only *derived from* the bath, represent the pivot of the experience.

But in the case of beautiful objects, it is the *object* which stands in the focus of my attention. *My own* affections drop out of its range, sometimes so completely that the question of liking or dislike, even pleasantness, appears almost absurd to the subject. However subjective the 'beauty' may be as to its original source, it *appears* as an objective quality of the thing, precisely because the self which invested it with it, has faded out of consciousness. The cleavage, as pointed out earlier, produced by the mixture of a subjective content with the objective thing is closed by the centralisation exercised by the object upon our mental attitude.

This change of the subjective adjustment from the beautiful to the agreeable can often be observed by a comparison, for instance, between our attitudes to music, as an aesthetic object, and music as a merely agreeable adjunct to some other pleasant occupation, as for example in table-music. In the one case we *listen to*, live in, and identify ourselves in a sense with, the music; in the other we eat and drink and talk, and incidentally *hear* the music.

Even things usually excluded from the beautiful and classed as only agreeable can, given favourable subjective conditions, become beautiful in themselves through this kind of centralisation, by an effort of attention and emotional imagination; such is the case with tastes, scents, organic sensations of health and buoyancy, purely muscular activity, &c., and similar experiences so enthusiastically claimed as beautiful by M. Guyau[1] in one of his best-known works.

This very sketchy exposition is in substance the psychological formulation of what has so long in Aesthetics been known as the 'autotelicity' of the aesthetic attitude, as distinct from the 'agreeable' which lacks this autotelic objectivity.

It is the same point at which Witasek[2] aims from the point of view of pleasure, when he observes that in aesthetic objects the *cause* and the *object* of pleasure coincide, while in other cases they are referred to different things (in the case of the bath, for instance, the *cause* is the bath, but the *object* my sensations).

On the basis of this rough distinction, the aesthetic evaluation of the four 'perceptive types' presents no difficulty.

As the type of lowest aesthetic value (if it can be said to have any at all) we should have to regard the *pure 'physiological' type*, *i.e.* that form of the type which remains strictly within the limits of the 'physiological' impressions without any tendency to develop these beyond 'physiological' effects into character-aspects. To subjects of this pure 'physiological' type colours are, in fact, merely 'agreeable,' but not 'beautiful'; they are the inducing factors for certain pleasant subjective affections, states of agreeable stimulation, comfortable warmth, &c. To quote again the previous illustration, they are absolutely on a par with the pleasantness of a warm bath.

---

[1] M. Guyau, *Problèmes de l'Esthétique Contemporaine*, Paris, 1897, 4me Ed. Livre I. Chap. VI.

[2] S. Witasek, 'Zur psychologischen Analyse der aesthetischen Einfühlung,' *Ztschr. für Psychol. u. Physiol. der Sinnesorgane*, Bd. XXV. S. 12, 18; *Grundzüge der allgemeinen Æsthetik*, Leipzig, 1904, S. 19–21.

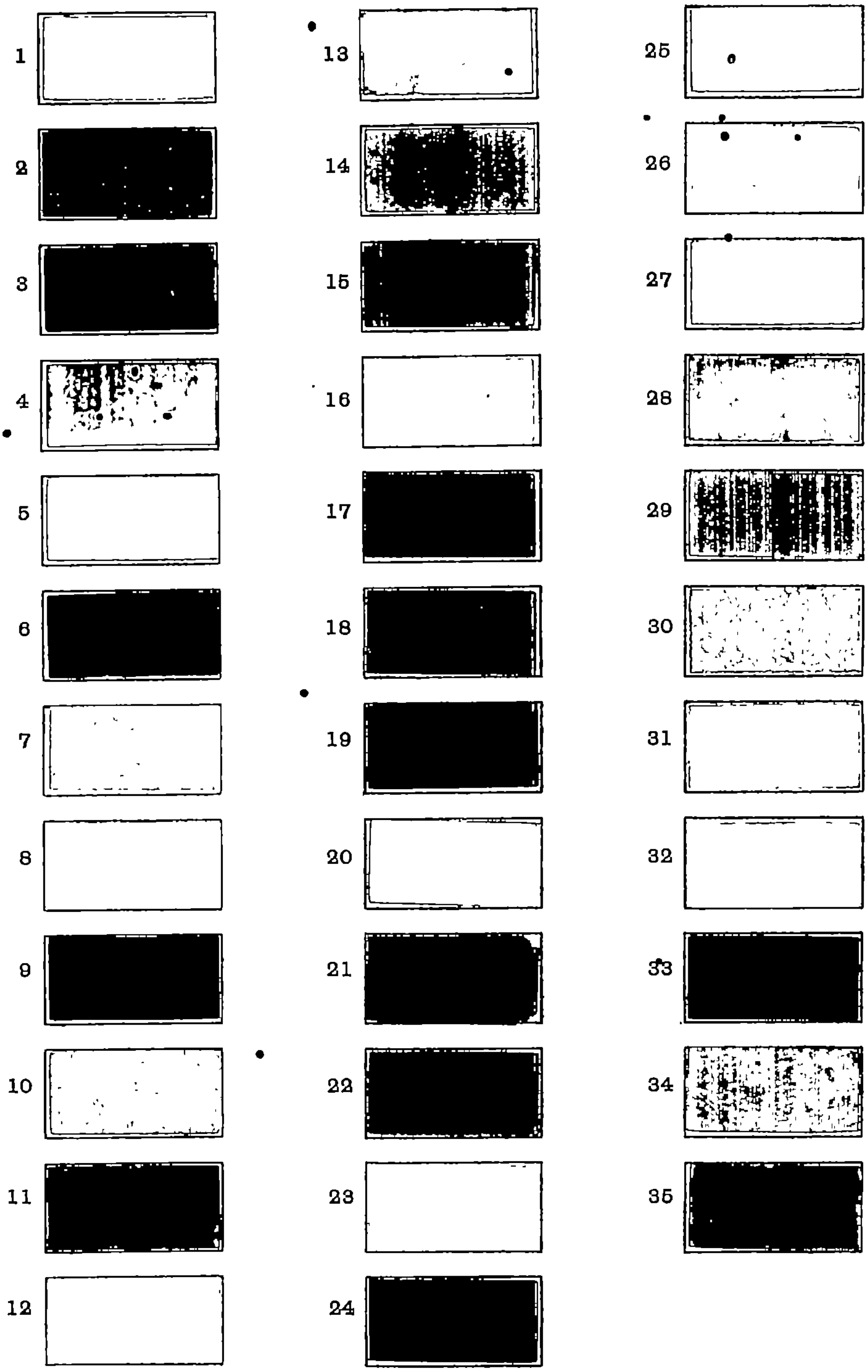
The same would apply also, with certain differences, to the *non-fused associative impressions* of some members of the associative type. The difference consists merely in that the pleasantness lies more in the sphere of psychical agreeableness than of physical well-being (if I may use this crude distinction); *i.e.* it is more a question of pleasant memories, pleasant emotional states induced by the colour-impressions, than of agreeable sensations of an organic nature. Otherwise the position of the colour-impression is just as marginal and that of the subjective affections just as focal in consciousness as in the case of the 'physiological' effects; there is no sign of a centralised unification of colour and affective reaction.

The next degree would be represented by the '*objective*' *type*. It can claim, on the one hand, a far greater emphasis laid on the colour-impression itself than either of the two previous classes. On the other hand, it suffers under the externality of its standard, its lack of appreciation of the colour as individual entity and the intellectuality of its judgment. It is a theoretical more than a practical attitude and shows its abstractness in the absence of personal sympathy with the thing it appreciates. A colour 'means' much less to this type than it does to the above-mentioned two groups.

With '*fused associations*' we enter the sphere of actually *aesthetic* appreciations. If it were not for the inherent instability of the fusion and for the danger of the reappearance of the old cleavage between colour and associated content, this type might almost represent the highest form of aesthetic colour-appreciation. But, owing to its origin, this danger is greatest precisely where appreciation—based upon the most 'subjective' and therefore most efficient associations—is most emphatic and personally most intense; the result is that presumably hardly ever a case can be found, where this ideal construction of a subjectively most powerful associated content in complete fusion with the colour-impression is actually realised.

This perfect unification and centralisation is to be found in the *character-type*. The very fact that the temperament or character, as the interpretation of originally purely *subjective* affections, is exteriorised and objectivated in the colour, and is thus, together with all its original emotional efficacy, severed from its connexion with the individual, represents the case of the most complete fusion of character and colour, together with the maximum of emotional potency. Neither the tempera-

PLATE X



These are the colours referred to in Table II. As their tone-value changes very considerably according to the illumination, it is essential that they be viewed in artificial light, incandescent gas having been used in the tests.

ment nor the colour exists independently of each other in the mind of the subject, but the perception of the one implies the consciousness of the other. Its freedom from purely personal factors, from accidental memories and irrational associations, and its essentially emotional tone invest this type with a kind of objective reality, which is generally characteristic of aesthetic experiences, and stamps this form of colour-appreciation as the *aesthetic* appreciation *par excellence.*

NOTE TO PLATE X.

The colour-names in Table II. are those given me by one of the subjects for the colours of Plate X., seen singly, under strong incandescent gas-illumination and with the exclusion of all other colours from the visual field. There were, however, surprisingly great differences in the colour-nomenclature used by different subjects. But this in no way invalidates the results deduced from the experiments.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

Jan. 25, 1908.	The Function of Images, by W. H. Winch.
Mar. 14, 1908.	Intoxication in relation to Religion, by J. H. Leuba. Displeasure, by A. F. Shand.
May 9, 1908.	The Application of Psychology to Education (two papers), by W. Brown and W. M. Keatinge.